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POST-ANÆSTHETIC TOXÆMIAS.

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Whilst medical men are acutely alive to the immediate dangers of general anæsthetics, but little attention is given to the remote effects which are none the less redoubtable if a little more obscure. Many administrators appear to think that their responsibility ends with the departure of the patient from the operating table, and yet from the patient's point of view a fatal result a few days later is equally disastrous. The question of immediate mortality has received considerable attention and is fairly accurately expressed by the late Sir Frederick Hewitt's figures. Chloroform 1 in 2,000—3,000; ether 1 in 16,000. In his time fatalities with gas and oxygen were practically unknown, but with the use of this agent in major surgery a number of deaths have occurred and its immediate safety in severe operations is probably about on a par with that of ether. These figures have received an exaggerated importance and considerable anæsthetic research has been performed with the hope of reducing them, but without decrying this in any way even if the ideal were attained only one life in many thousands could be saved whereas if attention were paid to the remote effects many lives could be saved in every thousand. To illustrate the importance of these remote effects two classes of cases will first be considered from the point of view of one's own personal experience :

1. Cases of cardiac failure following general anæsthesia.
2. Cases of post-anæsthetic toxæmia in children.

1. *Cases of cardiac failure following general anæsthesia.*

About the beginning of the present century it was largely the custom to use chloroform or chloroform mixtures in abdominal operations and efforts to introduce ether for this purpose were strongly opposed by many surgeons. One had ample opportunity accordingly of observing the effects of this anæsthetic in acute abdominal cases. The immediate effects were perfectly satisfactory, the patient usually took the anæsthetic well and the flaccid abdomen and quiet respirations facilitated the surgeon's work. Enquiry into the after-course of these cases did not reveal so gratifying a state of affairs especially in cases of acute appendicitis with abscess formation and peritonitis. Investigation of unfavourable cases usually revealed the following sequence of events:—The day after the operation the patient was perfectly well, on the second day after operation they were still quite well, on the third day the patient was not so well and the surgeon did not like the pulse. About the third or fourth day many of these patients died, and the only available information as to the cause of death was that "the pulse went." On questioning house surgeons as to the course of similar cases in hospital an almost identical statement was obtained. What was the explanation of these deaths? All these cases before operation were suffering from an acute intoxication with cloudy swelling and a certain degree of fatty degeneration of the liver, kidneys and heart muscle. These effects are lessened in varying degrees by the development of the patient's own antibodies. Whilst the removal of the appendix and drainage removed the source of the toxæmia, the administration of chloroform produced two highly detrimental effects:—

(*a*) It led to the production of acidosis (or ketosis) which largely neutralises the effects of the patient's antibodies. Thus although the cause of the toxæmia had been removed by the surgeon, the existing toxæmia was enormously intensified and the damaging effects of the toxin on the cellular organs still continued, nature's defence being temporarily removed.

(*b*) Chloroform itself is a protoplasmic poison and tends to produce fatty degeneration of these same organs.

These effects reach their maximum about the third day just as the patient is beginning to sit up and move about in bed. A sudden movement or the use of the bed pan or some such similar incident and the pulse fails. The cause of death in these cases is cardiac muscle failure due to the combined action of an existing disease and chloroform.

Since the introduction of ether for this class of case the after-mortality has been enormously reduced, and scarcely any case which receives adequate surgical treatment need be despaired of. The only safe procedure in these cases is to exclude chloroform in any form. Chloroform mixtures are equally injurious, as the following illustrative case clearly indicates.

The case was reported to me by the doctor in charge and my opinion asked as to the cause of death.

The patient was a servant girl of 25 years of age, whom he had attended for an attack of simple acute appendicitis. The attack appeared to be subsiding but as some tenderness still remained he decided to call in a surgeon. Removal of the appendix was decided upon, and the patient was removed to hospital. The patient was anæsthetised with A.C.E. and the anæsthesia throughout was uneventful; an inflamed appendix was found but no pus. The operation was completed in about 30 minutes. Progress was perfectly normal in every way until the third day when the patient sat up in bed and suddenly died.

The case occurred in 1916, and I ventured an explanation on the lines indicated above. Fortunately a post-mortem examination was obtainable and revealed nothing abnormal save a fatty change in the liver and heart muscle. Clinically the development of this particular form of toxæmia is indicated by the development of a hot dry burning skin during the course of the operation, so conspicuous that it thrusts itself upon one's attention as one's hand lies upon the patient's face. On more than one occasion I have noticed it when no septic focus was expected, and on drawing the surgeon's attention to it further search has revealed the presence of an intra-peritoneal abscess. More recently one has noticed another class of case in which cardiac failure following general anæsthesia is prone to occur. I refer to long-standing cases of

urinary obstruction in which the existing toxæmia seems to have produced such degeneration of the cardiac muscle that the administration of a general anæsthetic other than gas and oxygen causes serious cardiac breakdown, if not actual syncope. Prolonged drainage of the urinary tract effects remarkable improvement and generally enables a more severe operation to be safely undertaken at a later date. The testing of the efficiency of the heart muscle in these cases has been dealt with in a previous paper in this Journal (Vol. I, No. 4, 1924).

Further observations on this condition are eminently desirable.

2. *Cases of post-anæsthetic toxæmia in children.*

In May 1917, I was asked to investigate a series of cases occurring at a Children's Hospital, showing serious symptoms after general anæsthesia, some of them terminating fatally. In about two months five deaths occurred and in four of these a post-mortem examination was obtained. Three years later five more similar deaths took place in a period of three months. Altogether observations were made on 700 cases, but in the present article only those which terminated fatally will be considered.

Whilst the classical form of delayed chloroform has been well established since attention was drawn to it by Guthrie in 1894, yet the present series of cases show various features of note, and are of special interest in that complete post-mortem examinations were obtained in the first four instances. For the reports I am indebted to Professor Dean, who was then at Manchester University, and Dr. Sellars, Pathologist to the Hospital. As space will not allow a complete description of each case, a summary of the outstanding features presented by the series will be given.

Age. The respective age of the patients in years was:—1, 1¼, 2½, 8, 3, 1, 1, 1½, 1, 1. Whilst children are admitted to the hospital up to the age of 15 years all the cases, with one exception, were under the age of four. Some of the older subjects exhibited signs of toxæmia but cleared up.

The Operation. The various operations performed were as follows:—Tenotomy for talipes equino-varus (double); osteotomy of femur for genu varum (double); tenotomy for

talipes equino-varus (single); removal of dermoid cyst from outer side of the orbit; hydrocele of the cord (single); osteotomy of femur for genu varum (double); radical cure of double inguinal hernia; radical cure of left inguinal hernia; radical cure of right inguinal hernia; radical cure of right inguinal hernia and circumcision for phimosis.

Most of the operations were completed in about ten to fifteen minutes, and there was nothing in connection with the operation or in the subsequent appearance of the wound which could account for the toxæmia.

The Anæsthetic. In seven of the ten cases chloroform or chloroform mixtures were definitely used, in one case the anæsthetic was not noted but the administrator concerned was in the habit of using A.C.E. In two cases ether alone was said to have been used, but unfortunately one cannot say that no chloroform whatever was given to the patient. The ether used was suspected of impurities but none were discovered, and the subsequent course of events did not favour this view.

The Temperature. All the cases had a normal temperature before operation and there was nothing in connection with their ailments or the operation to account for an appreciable rise of temperature. Observations on several hundred normal cases in children showed that a rise above 99½° F. was extremely rare and when marked was generally a warning of commencing infectious disease or of septic changes in a wound.

In the fatal cases the temperature rose to 104°—105° just before death with one exception in which it remained about 99½° throughout. In one instance the temperature reached 108°. As a rule there was a gradual rise of 1½° for the first two or three days and a rapid rise towards death. Apparently this sudden rise towards the end is to be explained by the convulsions which were constantly manifested. The notes on the case where no appreciable rise of temperature occurred state that the child exhibited shock from the beginning and that the convulsions occurred only half an hour before death, and the final temperature record is not available.

The Pulse and Respiration. Acceleration of the pulse was a constant feature, and showed itself almost from the commencement. The average rate for the first 48 hours was 130—

140. Further acceleration took place as death approached up to 160—180—200. The respirations were also increased but not relatively so much as the pulse, the average rate being 30—40.

Vomiting. This symptom was very variable and showed considerable variation from the typical form of delayed chloroform poisoning usually described. In four cases there was no mention of vomiting at all; in two others there was slight initial vomiting limited to the first 24 hours, indistinguishable from slight post-anæsthetic vomiting; in the remaining four instances there was no initial vomiting, but this symptom became almost continuous, commencing at periods of 24, 36, 48 and 72 hours respectively. In no instance was the so-called "coffee-ground" vomit a conspicuous feature.

Restlessness. This feature was pronounced in every case, showing itself within twelve hours and increasing in severity as the condition became worse.

Jaundice. Jaundice was not noted in any single case.

Acetone in the breath. In two instances this manifestation was detected, in one case slightly, in the other it was pronounced. In most cases some ether was given as well as chloroform, and under these conditions it is difficult to distinguish between the ether excreted in the breath and slight traces of acetone.

Convulsions and Coma. Convulsions were present in every case and were especially pronounced towards the end. They terminated in coma or exhaustion usually about the third or fourth day.

The Urine. Ferric chloride was used as the test for ketone bodies in this series, and in only one instance was there a positive reaction before operation, and this was only slight. After operation three cases showed slight traces of ketone bodies. Five cases gave a very pronounced reaction during the first 24 hours but the amount diminished progressively towards the third and fourth days. Whether this indicated impaired excretion by the kidneys or whether they were replaced by some substance which did not give the above reaction it is impossible to say. In two instances specimens of urine were not obtainable after operation. Albumin was distinctly present in three of the cases; in one of these the

amount was excessive and urinary suppression occurred. In this last case as no urine was passed a catheter was introduced into the bladder but no urine was obtained until the third day; the specimen on this occasion was loaded with albumin but showed no reaction with ferric chloride.

Duration of the condition. In six cases death occurred on the third day. In two cases death occurred on the fourth day. In one case death occurred on the second day. In one case death occurred on the seventh day.

Post-mortem appearances. The appearances constantly found in the four instances in which a post-mortem was obtained were as follows:—

Brain. Œdema and marked congestion of the meninges.

Liver and kidneys. Pale, and showed fatty degeneration.

Heart. Usually a little dilated otherwise normal to the naked eye, but showed microscopically disappearance of striation and fatty degeneration. One case exhibited a patent foramen ovale.

Lungs. For the most part normal, but two cases showed congestion but no evidence of pneumonia.

Thymus. This organ was invariably large and well-formed, but as all the cases were under four years of age could not be considered abnormal.

Digestive system. The stomach was invariably dilated, but nothing else abnormal was noticed. In one instance the large intestine possessed a complete mesentery.

Post-mortem staining was well marked.

Professor Dean reported that the liver, kidneys and heart muscle all showed well-marked signs of toxæmia, but that he could throw no light on the cause of the toxæmia.

(*To be continued*).

At the recent Congress of the Associated Anæsthetists of the United States and Canada, a Silver Loving Cup was presented to Dr. Wesley Bourne, President, in recognition of his research work, and especially for his organisation of the Canadian Society of Anæsthetists. The Scroll of Recognition was presented to Profs. Yandell Henderson, H. W. Haggard, A. W. Rowe, and W. L. Mendee-hall for Meritorious Researches in Anæsthesia and Analgesia.

THE ADVENT OF A GREAT DISCOVERY: OCTOBER, 1846.

By Dudley Wilmot Buxton, M.D., M.R.C.P.

LIKE most great discoveries, that of anæsthesia was the outcome of the work of many men who laboured to a common end, the importance of which they only partly appreciated. Looking back through years which have lapsed since October 16, 1846, when Morton publicly demonstrated the possibilities of inhalational anæsthesia by ether, we can review the question to whom belong, by merit, the palms of that discovery. It is certainly deplorable that enthusiasm for one or another claimant should have led to unfair depreciation of the work of others. Science knows no loyalty save to Truth, no partiality but to those workers who strive for the truth without thought of ultimate guerdon or self-seeking.

Sir Benjamin Brodie wrote in 1846: "Physicians and surgeons have been looking in vain from the days of Hippocrates to the present time for the means of allaying or preventing bodily pain." The history of anæsthesia in its earlier phases, when mandragora, opium, cannabis indica, lettuce used in potions, compression of the carotid arteries and other means of exsanguinating the brain centres does not fall within the ambit of this paper, interesting though may be the story. The point I wish to make is that the medical mind was gradually opening to the possibility of painless surgery. Ambroise Paré, Avicenna, Averrhoes, Serapion and many more refer to draughts of narcotics and inhalation from sponges steeped in decoctions of herbs, but no general acceptance of the idea of anæsthesia or of a practical means for its production were introduced to the profession before the great awakening of science which followed the work of the chemists Hales, Priestley, Lavoisier, Cavendish, and those who laboured with them in the eighteenth century.

However, there was one potent influence at work which, while it purported to solve the problem how to obtain surgical anæsthesia, really, as we shall see, darkened councils and hindered the adoption of scientific methods for the production of inhalational anæsthesia. This was hypnotism or mesmerism or the neurhypnotic trance, as it was termed, which came into vogue with Friedrich Anton Mesmer (1734—1815), who published his book, "The Influence of the Planets in the Cure of Disease," in 1776. Cardan's teaching that the magnet could cure disease; the exploitation of "animal magnetism" by Glocenius, Burgrave and Paracelsus had led the way for Mesmer's practice. Whatever fallacies and impudent assertions marred the presentation of the discovery that hypnotism produces unconsciousness to pain during surgical operations, the fact remains that Mesmer in his hospital in Paris, where Cloquet operated, demonstrated the truth of this statement. Braid, of Manchester, in 1841, Esdaile, in India and later in England, and Elliotson, at University College Hospital, believed in the theory and practice of hypnotism, even if they refused credence to the bombastic egotism of Paracelsus and Mesmer.

A Royal Commission composed of English and French medical savants, appointed to enquire into the validity of Mesmer's claims, was unanimous in condemnation of Mesmer and all his works. Dr. Elliotson was removed from his teaching appointment. As we should expect, the laity of the civilized world thereupon took hypnotism to its bosom as a desirable form of quackery. It had been gloriously advertised, and, being condemned by most of the profession in Great Britain and the States of America, became at once widely known and extensively adopted. Thus at the time when the discovery of inhalational anæsthesia was in the womb of Fate and coming to the birth, mesmerism held the public suffrage and was to prove a powerful rival to the bantling anæsthesia.

The steps of our history should take us to the story of nitrous oxide, the success and failure, for the nonce, of that gas for producing anæsthesia. However, we must pass by this page of our story and attempt to trace the history of the

narcotic which the world has learnt to regard as the pioneer of modern-day anæsthesia—ether, although nitrous oxide was certainly the first drug to induce anæsthesia by inhalation.

It is commonly believed that the chemists obtained their first knowledge of ether from an Arabian, one Djafar Yeber. Nor is this statement improbable, for we certainly learnt the method of distillation from the East, and the words alchemy, alembic, alkali, and alcohol demonstrate that our early scientists went to the Arabs for much of their chemical lore. Let us not forget that alchemy begot chemistry.

Ether was known to Raymond Lully (1234—1315), an enlightened doctor who spent many years in his attempts to convert the followers of Islam, but they eventually stoned him to death, thus marking their appreciation of the gentle doctor's efforts on their behalf. In 1540 Valerius Cordus named the drug Oleum Vitrioli Dulci, and described the method of preparing it from sulphuric acid and alcohol. At the end of the fifteenth century Basil Valentine, the alchemist and reputed author of "The Triumphal Chariot of Antimony" (which was translated in 1661), referred to ether. Binz, in his "Lectures on Pharmacology," says: "As regards experiments on animals, I find a passage in the works of Paracelsus, who died in A.D. 1541, which must refer to sulphuric ether, and in which he recommends its internal use in preference to opium for the relief of pain." Again, Conrad Gessner, in 1552, published a full account of how to prepare it. According to Mr. Foy, to Dr. Michael Morris should be given the credit of having explained the method of preparing sulphuric ether, and even of suggesting its use by inhalation. However, Dr. Morris's paper is published in the "Medical Observations and Enquiries, by a Society of Physicians in London, Vol. ii., in 1758," a comparatively recent date. Frobenius named the drug Sulphuric Ether in 1730.

With Dr. Thomas Beddoes (1760—1808) we are introduced to fresh and important developments in the history of ether anæsthesia. Beddoes, the son of a tanner, was a man of light and leading. He went to Pembroke College, Oxford. In due course he took his M.D. degree, went abroad, and, having studied chemistry under Lavoisier, returned to Oxford and was appointed University Reader in

Chemistry. However, he was a publicist as well as a physician. He published his "Rights of Man" and "Encouragement of Quackery by the Clergy," which tractates did not endear him to the Oxford authorities, and at their instance Beddoes left his *alma mater* and decided to devote himself to Humeral Pathology and the treatment of disease by the inhalation of drugs.

In 1798, with the assistance of Thomas Wedgwood, a son of the renowned potter, Beddoes started his Pneumatic Institution at Clifton. Here he worked with James Watt, the engineer, and with the assistance of Humphry Davy. Dr. Beddoes repeated experiments originally undertaken by Thornton, who had employed the inhalation of ether vapour successfully to relieve pain and oppression in the chest of persons suffering from pulmonary disease. On one occasion he made a woman unconscious by ether inhaled by her to relieve the distress caused by disease of her breast. Beddoes, by the publication of a journal under the name of "Contributions to Medical and Physical Knowledge from the West of England," ventilated the work of the Institute and incidentally revealed the possibilities of inhalation in assuaging pain.

Dr. Pearson, of Birmingham, and Dr. Woolcombe, of Plymouth, advised the inhalation of ether to relieve pain due to lung disease, and the former published an account of his results. The curious will find a eulogistic mention of Dr. Woolcombe's work in the pages of Sir Thomas Watson's classical Lectures.

Davy, in the course of his researches on "Factitious Gases," had discovered the exhilarating effects of nitrous oxide, and had even suggested the use of this gas as an anæsthetic. However, no one was found to investigate the matter. Henry Hill Hickman experimented with the same end in view early in the nineteenth century, and when he expounded his results he was laughed at for his pains. "Those whom the gods love die young"; Hickman was but 29 at his death in 1829.

In 1818 Faraday discussed the possibilities of ether, and compared its exhilarating and narcotising effects with those of laughing gas. He cautioned against the inhalation of

ether vapour unless the greatest care is exercised, as fatal results had followed the practice.

Orfila, who is termed the "founder of toxicology," and whose work, "Traité de Toxicologie Générale," was published in 1813, experimented on dogs with ether, and described this drug as a poison, placing it in the same class as alcohol. Christison, who was Orfila's pupil, gives a fairly comprehensive account of ether in his classical work on "Poisons" (1829), although he admits that little was known about the effects of large doses of ether given to man. He mentions a case of intermittent lethargy lasting thirty-six hours, due, he believed, to a too prolonged inhalation of ether. Further, Christison quotes two deaths due to ether which occurred in 1830. One, that of a druggist's maid-servant, who was found dead in her bed in a room in which a three-gallon jar of Spiritus Etheris Nitrosi had become broken, filling the apartment with the vapour of Nitric Ether. The second case was quite similar. A servant sleeping in an ill-ventilated room was asphyxiated through the bursting of a carboy of nitric ether.

Michael Faraday, the physicist, writing in 1818, describes ether as possessing similar narcotic powers to nitrous oxide gas, and suggests its possible use as an anæsthetic. A statement important from its emphasizing the fact that ether was commonly used in convivial gatherings as an exhilarant, is made in Mitchell's "Chemistry" (quoted in Beck's "Jurisprudence," p. 944; Albany, 1835). "Some years ago a practice obtained among the lads of Philadelphia of inhaling the vapour of sulphuric ether by way of sport. A small quantity, placed in a bladder, was almost instantly converted into vapour by the application of hot water. By means of a tube and stop-cock, the gas [vapour] could be easily inhaled. In some instances the experiment excited mere playfulness and sprightly movements, but with other cases delirium and even phrenitis was induced, which ended fatally." It would appear that the apparatus was similar if not the same as that adopted by Morton in his demonstration on October 16th, 1846. Beck's book, the sixth edition, appeared in 1838, was very widely known, and there can be little doubt that Dr. C. T. Jackson, one of the claimants for the priority of

discovery of ether as an anæsthetic, must have been well acquainted with its properties. According to Poulsson,* "in 1841 its (ether's) anæsthetizing properties were discovered quite accidentally by Jackson in Boston, when, after the explosion of a vessel containing ether, the laboratory assistant was found anæsthetized." Several deaths attributed to ether were published, and Jackson is said to have made some experiments to determine whether large doses could be taken safely. It would appear that Beddoes also investigated this question, but did so from its medical aspect. Both Professor Turner, of University College, London, and Professor Thompson, at Glasgow, were in the habit of giving the members of their classes in Chemistry, ether to inhale in order to demonstrate its exhilarating and narcotic effects. This was in 1835. We are told that a student named Robert H. Collier, and a member of Professor Turner's class, was rendered unconscious through inhaling ether and some four years later lectured on the narcotizing powers of the alcohol group, but associated their action with that of mesmerism. There was a reported operation—tooth extraction—under ether said to have been performed by a medical student, W. E. Clarke, in 1842. He claimed to have attended an "ether frolic" in 1839 at Rochester, New York, and to have been induced by his experiences to experiment with ether. Dr. Marcy also claimed to have administered ether to a patient for a minor operation. This was in Hartford in 1844, and so Dr. Marcy would have known of Wells's experiment with nitrous oxide. It is clear, however, that none of these freak experiments were conducted upon a scientific basis nor led to any material advance in the knowledge of anæsthesia.

So far as ether is concerned, the first material constructive work in the practice of anæsthesia is undoubtedly that of Crawford Williamson Long† (1815—1879). Long sprang from an intellectual family, and was himself a brilliant student, an expert surgeon and a high-principled man. After

* *Pharmacology and Therapeutics*, p. 8, 1923.

†See papers by the present writer, *Proc. of the Royal Society of Medicine*, 1912, vol. V (Section of Anæsthesia, pp. 19—45), and "Sleep and her Twin-Sister Death," *Contemporary Review*, Jan. 1st, 1913.

graduating at the Pennsylvania University he commenced general practice in 1841 in Jefferson, Georgia, at the age of 26. As has been indicated above, mesmerism was at this time at its zenith of popularity and was accepted as the established means whereby anæsthesia could be obtained. Braid's researches, Esdaile's work in India, Elliotson's eloquent advocacy in London, and Richerand's adoption of hypnosis in France, all contributed to this end. In 1842 Long was asked to supply nitrous oxide for some youths to indulge in a "frolic." Failing this drug he gave them some ether, and observed, as did Horace Wells two years later in the case of nitrous oxide, that those who had inhaled the ether were wholly unaware of bruises and cuts incurred in their intoxicated gambols. Then follows the story, now matter of history, how Long made use of ether in his surgical practice with uniform success. The sworn evidence of these doings leaves no loophole for doubt of the accuracy of this statement. The story of Wilhite, that he suggested the use of ether to Long, must be discredited, firstly because Wilhite publicly confessed that his story was a pure fabrication, and because he (Wilhite) did not come as a pupil to Dr. Long until 1844, and Long's operations had been performed from 1842 onward.

Although widely known in his home neighbourhood, the discovery by Long of ether as an effectual means of producing surgical anæsthesia was not given to the world until after Morton's public demonstration before the profession on October 16, 1846. This is accounted for by the following facts. Dr. Crawford Long, with the true instinct of the scientist, waited, but waited too long, for more experience and more surgical opportunities to test his discovery. He was anxious to exclude the possible influence of hypnotism, then so widely accepted, to explain new phenomena. Those to whom he appealed for guidance urged upon him the recognised dangers of ether inhalation, no doubt exaggerated, as its true physiological action was little known, and further pointed out that mesmerism fulfilled all the requirements of the surgeon and was perfectly safe. Further, Dr. Long was a very busy practitioner, having little or no leisure for writing to the professional journals. The attitude of

Dr. Crawford Long was undoubtedly correct, even if mistaken, and though we cannot accord him the honour of having published to the world the discovery of anæsthesia by ether, we must recognize that he was, in fact, the first surgeon who appreciated the value of the method and adopted it in his own practice. Equally was his attitude correct and high principled when in the later days the question of the State recognition of the invention of anæsthesia was mooted and Dr. Jackson approached him with the view of making a combined appeal for public recognition and a monetary grant. Dr. Long declined to join forces with Jackson, produced the evidence of his personal work, which he placed in the hands of his legal adviser, instructing him merely to lay them on the table, making no claim, pleading no merit save that of priority of actual accomplishment. Out of the welter of the ether controversy Crawford Long comes with clean hands and an untainted ethical standard. He wrote to Jackson: "Our claims are rival, and permit me, sir, to say that although our claims are conflicting, I would not knowingly say anything in an article (he was preparing a full statement of his work in the form of an article) which would be displeasing to you. . . . Still it becomes each one of us to use all honourable means to advance his own claims, and I know you will not blame me for attending to this matter which so much concerns my reputation." This statement referred to as an article was made to the Medical Society of Georgia in 1849. Long summarized his attitude in the matter in the following words: "My only wish about it is to be regarded as the benefactor of my race."

It is acclaimed that W. T. G. Morton introduced anæsthesia to the world in 1846. He was not the first to use ether, nor was he the first to suggest inhalational anæsthesia. Crawford Long first used ether in surgery, and Horace Wells first claimed that nitrous oxide was capable of producing surgical anæsthesia. However, Long did not invite the suffrage of the medical and surgical faculties by promptly publishing his results and Wells failed through an error in technique to substantiate his claim for the efficiency of nitrous oxide as an anæsthetic. So it was that Morton successfully demonstrated the surgical value of ether, which

he disingenuously termed "Letheon," and did so to an adverse-minded audience, but he succeeded in his great adventure. *Palmam qui meruit ferat.*

The Morton narrative is trite, but may bear a brief comment. The history of William Thomas Green Morton (1819—1868) is not easily traced, as many discrepancies and contradictions have been introduced into the published accounts of his work. As a dentist and partner with Horace Wells at Hartford, Morton would naturally have watched the nitrous oxide exploitation of 1844. It is even said that he was present in the Massachusetts General Hospital when Wells's demonstration failed and he was booed out of the theatre.

The metallurgical chemist, Dr. C. T. Jackson, who, as we have seen, had learned in a somewhat dramatic fashion the narcotic powers of ether, had also proved its anæsthetic effect by inspiring it after the inadvertent inhalation of Chlorine gas, and so was recognized as an authority on these matters. To him, after separation from Wells, Morton repaired and sought his advice and assistance. There is no doubt that Morton thoroughly appreciated the advantages of painless dentistry, and enthusiastically set about discovering how to accomplish this. Jackson appears to have deprecated the adoption of mesmerism, for we must assume that if the statement is correct Morton sought to borrow a gas-bag which he proposed to apply to a patient's face. Such a procedure could only act by " suggestion " if the bag contained merely air. He proposed ether in lieu of nitrous oxide, since the latter was out of favour at this time owing to Wells's fiasco. Morton adopted Jackson's suggestion, and, according to the latter, received help and guidance from him. Be that as it may, Morton chose the highly meritorious plan of experimenting, and did so upon the lower animals and upon himself. Subsequently he used ether in his dental practice, extracting teeth painlessly. Then, on October 16, 1846, came the crowning triumph of Morton's labours, he gained permission to administer " Letheon," as he called his doctored ether, and Mr. Warren, the well-known surgeon operated. We are told that the patient, although not completely narcotized, admitted that he felt no pain. Subsequent attempts were unequivocable successes, and Morton was

acclaimed the inventor of the greatest discovery of the age.

Professor Louis Agassiz, according to a report of a meeting of the Boston Academy of Arts and Sciences, when the question of the discovery of ether anæsthesia was discussed, challenged Dr. Jackson, demanding, "Did you try any experiments with ether?" "If the first patient etherized by Morton had died, would you have accepted any share in the responsibility?" and no answers were given. If the statement is true, it is really not remarkable that a chemist who was mainly engaged in geological chemistry should leave the practical side of the ether test to the man to whom he had imparted the current knowledge about the drug, and to whom he had recounted his own experiences. Claims were advanced by Wells, by Morton and by Jackson for State recognition and for substantial financial grants as rewards for their work on anæsthesia. The question of monetary reward, however, was shelved in the States owing to governmental delays and the unloosing of the dogs of war, and it was left to France to recognize at that period the merit of the labours undertaken on behalf of mankind by granting the Montyon prizes of 2,500 francs—one to Jackson for the discovery of etherization, and one to Morton for its application to surgical operations of the discovery.

MORTON AND THE MALINGERER.

When Morton, at the request of a Congressional Committee, appeared before that body at Washington to give actual demonstrations of his discovery, one of the cases that he took over was that of a private of artillery who was thought to be a malingerer. The alleged condition for which he was applying for a discharge from the army was an anchylosis of the knee. Army officers had suspected the soldier of malingering but had failed to make this belief positive.

In the presence of a large number of Congressmen Dr. Morton administered the ether to him, and, as the anæsthetic became effective, of course, the feigned muscular rigidity disappeared. He was then allowed gradually to resume consciousness and, having been placed in the erect position, was led around the room. Suddenly, the fog having cleared from the man's brain, and, realizing that he had "let the cat out of the bag," he gave way to uncontrolled rage, and it required several attendants to protect Dr. Morton from assault by the exposed and disappointed artilleryman.

EYE TROUBLES AND ANÆSTHESIA.

By Joseph Blomfield, O.B.E., M.D. (Cantab.).

EYE troubles following anæsthesia have not received the attention which they deserve. The text-books give but few words to these occurrences and they figure very rarely in the periodic literature. Nevertheless, these accidents should not be ignored, for, on the one hand, they are almost entirely preventable when their possible appearance is well borne in mind; and, secondly, they are no light matter to the patient concerned. One reason perhaps why so little attention has been given to the subject is that in the majority of instances eye trouble due to an anæsthetic is only of a minor variety—a transitory conjunctivitis, for example. Now and then, however, more serious damage is done, and this has led not only to permanent defective vision in the eye, but on more than one occasion to serious litigation in which the anæsthetist has been involved as defendant. Obviously, therefore, the subject is well worthy of the attention of anæsthetists. This is so, for another reason, and that is that damage to the eye appears to be far more commonly sustained since the prevalence of open methods of administering anæsthetics than it was in the days when "closed ether" or "gas and ether" was the usual routine. This brings us to consider in what way the damage is generally brought about. As regards the eye and its use during the two methods, open and closed, we can note at once two important differences. The eye is apt to be consulted both by finger and by inspection much more often during an open than during a closed administration; and, secondly, anæsthetic vapour has much easier access to the surface of the eye during the former than during the latter forms of administration. What damage can the mere anæsthetic vapour bring about? It seems quite certain that not only chloroform vapour, but also the vapour of mixtures and even that of pure ether, can cause acute conjunctivitis by their contact with the anæsthetic eye. And the great majority of cases of "pink eye" after anæsthetics are probably

brought about in this way. Consequently we learn the lesson that during these open administrations the eyelid should be carefully kept down and the eye consulted as rarely as possible. In this way alone can the vapour be prevented from reaching the conjunctiva. Every anæsthetist of experience uses the eye, of course, only rarely during administration, being quite aware without its aid of the exact state of the narcosis he is producing. The novice, however, is very prone to look to the eye as his first and most important guide. As teachers we should discourage this practice far more than it has been discouraged in the past. The most serious damage that has occurred to eyes after anæsthesia has been keratitis, with subsequent scarring of the cornea, and consequent defective vision through the scar passing across the pupil. The question arises whether such damage can ever be brought about by the inflammation set up by vapour alone, or in such cases must we always assume that either liquid anæsthetic has entered the eye or else the cornea has been abraded by the examining finger or by parts of the anæsthetist's apparatus. So far as the opinions of ophthalmologists of repute, and so far as a small amount of experimental evidence carries us, we must refuse to believe that the vapour itself can ever be responsible for corneal ulceration. It appears that ammonia vapour alone among familiar vapours can produce damage of this severity. Nevertheless the trouble has been confidently attributed to the anæsthetic vapour when damage to the eye supervened after severe operation on a man in a condition of severe toxæmia. The contention was that in such a bodily state the damage inflicted by vapour merely was enough to lead to scarring of the cornea, the general condition of the patient rendering the comparatively mild injury capable of producing the serious result because of all lack of resistance. It has been recommended that a drop of castor oil should be inserted in the eye at the beginning of anæsthesia, as a precaution against conjunctivitis. Other authorities recommend washing the eye with boracic lotion as a routine measure at the end of administration. The probability is that careful closing of the lids, and keeping them carefully closed throughout, is a surer way of averting trouble than either of the above practices.

THE PRESENT STATUS OF LOCAL ANÆSTHESIA.

By Robert Emmett Farr, M.D., Minneapolis, Minnesota, U.S.A.

I HARDLY need to suggest that the medical profession is to-day, as it has been since the discovery of anæsthesia, interested in improvements in anæsthesia. Great as was the advance made by the introduction of drugs for the purpose of producing general anæsthesia, there have always been associated with the method elements of danger and unpleasantness, the avoidance and the reduction of which have constantly confronted the profession. Noteworthy strides have been made, and yet even to-day no one would have the temerity to suggest, for instance, as did Ambrose Pere in the sixteenth century, when discussing the art of surgery of his time, that the art of anæsthesia had in any sense reached perfection.

Notwithstanding the comparative excellence of anæsthesia at the present time, the strife for safer and more pleasant methods goes on with special vigour and without abatement. Incidentally it may be noted that enthusiasm in relation to every new anæsthetic, or method, immediately assumes an acute upward curve which may continue at a certain level for a time, only too often to be followed by a gradual downward course.

It is the writer's belief that the ideal general or local anæsthetic has not yet been found. Perhaps Ethylene may fulfil the requirements. Until these requirements are fulfilled without the possibility of question or doubt it may be worth while to keep in mind the personal equation and to suspend final judgment, during which time those of us who have used local anæsthesia successfully may be given an opportunity to demonstrate some of its advantages and detail the manner in which these advantages may be made available.

As suggested, the literature will show that the introduction of every new general anæsthetic has been hailed with enthusiasm, the eclipse of which has gradually become manifest

as its successor made its appearance above the horizon. The fact is also patent that only after the introduction of some new inhalation anæsthetic have the shortcomings of its predecessor been thoroughly and distinctively demonstrated. Not infrequently it will be found that it has been the proponents of a method who have presented the belated statistical evidence.

No comprehensive discussion of local anæsthesia can ensue without referring to general anæsthesia, and, in discussing their relative merits, it is well to remember that the practical use of general anæsthesia preceded that of local anæsthesia approximately forty years, and that in a comparison of the methods this factor must be taken into account, as the lapse of time is an important element in improving the mode of application of any form of anæsthesia.

There are many points which must be considered in comparing the two main forms of anæsthesia. Regarding general anæsthesia—notwithstanding what drug or combination of drugs is used—the supervision of the method of administration assumes a rôle of the greatest importance, and it is an indisputable fact that the elimination of this factor greatly modifies any evaluation of the merits of the method. It is no fairer to consider the safety of a gastrectomy, for instance, without concomitantly estimating the ability of the individual who performs the operation, than it is to apply the same test, in the abstract, to the merits of general anæsthesia.

While it is not my purpose to discuss at any length the alleged disadvantages of general anæsthesia, the problem cannot be viewed in the broader sense without referring to at least two points: First, the actual dangers, inconvenience, distress, etc., associated with its administration, and, secondly, a comparison of the amount of surgery which is performed under skilfully administered inhalation anæsthesia with that where the reverse is true. With regard to the first point, I feel certain that most authorities agree that local is safer than general anæsthesia, and in my hands, at least, the patients suffer much less discomfort, distress and inconvenience when operated upon under successful local anæsthesia than I have ever found to be the case when general anæsthesia

was used. Secondly, it is my belief that a safe estimate would place the number of cases in which the administration of general anæsthesia is inadequately supervised extremely high, especially in the United States. In these cases there is usually no definite record, whereas what evidence we have is compiled by those who are in a position to show the results of general anæsthesia at its best. It is a fact that, relatively speaking, but a comparatively small percentage of surgical patients receive the benefit of skilled general anæsthesia. Thus the problem resolves itself mainly into a question of the greatest good to the greatest number.

If we admit the foregoing premise in relation to general anæsthesia we have at once established a more or less firm foundation for the necessity of improvement in general anæsthesia methods and have offered to the protagonist of local anæsthesia good and sufficient grounds for the presentation of his case without the necessity of referring more specifically to the other admitted disadvantages of general anæsthesia.

In discussing the present status of local anæsthesia it is thus obviously necessary to consider the factors mentioned above which relate to general anæsthesia. As we consider these the comparative *safety* of local anæsthesia becomes the outstanding feature. Its use by surgeons of repute the world over in the case of the hazardous risk should leave no argument necessary regarding its safety.

Considering the advantages of local anæsthesia in the broader sense and comparing them with those of inhalation anæsthesia which is more or less handicapped for the reasons just enumerated, the question of the possibility of the control of its administration becomes of prime importance, and here I would stress the fact that the successful administration of local anæsthesia is, and must necessarily be, in the hands or under the direct supervision of the surgeon himself. This most important factor places general anæsthesia at once upon the defensive and more or less effectually refutes one of the alleged shortcomings of local anæsthesia which will be referred to later in more detail—namely, the psychic reaction to the local anæsthesia method. We know that the administration of local anæsthesia is, and will be, for a considerable

number of years at least, in the hands of those best qualified to use it. When used with ordinary skill its toxicity is undoubtedly lower than is that of general anæsthesia, not only in its immediate, but in its remote effects as well. Were not these advantages outstanding, the fact that its employment is accompanied by, and demands, less trauma to the individual in general, and to the local parts in particular, this method would still present advantages with which we must reckon. Not the least of these is the opportunity allowed for the free ingestion of liquids, and, in many instances, nourishing food, during the immediate pre-operative period. Furthermore, when the operation has been performed under local anæsthesia the patient is, as a rule, left in a condition where liquid and solid nourishment may be ingested and assimilated at an earlier period than is the case when general anæsthesia has been administered.

In the science of medicine the realization of what is best and the standardization resulting therefrom is often a slow process. There is no question but what the surgical patient of to-day receives better care than during the past. For this, the use of local anæsthesia may, we believe, be credited at least to a certain extent. The influence of that group of surgeons who have been thinking and acting along the lines demanded by the use of the local anæsthesia method has, without question, been the means of calling the attention of surgeons to the necessity of greater respect for what the patient has a right to expect and demand than has heretofore been the case. It has been one of the factors that has made surgeons realize more keenly that individualization is essential in the pre-operative stage. We would refer especially to the psychic care of the patient during this period and to the necessity of a complete differential diagnosis. Similarly, during the operative stage, local anæsthesia has demanded of the surgeon a respect for the psychic and physical comfort of the patient and a refinement of technique, with an avoidance of general and local trauma, which in our opinion would have been greatly delayed had general anæsthesia alone occupied the field. And to go a step further, we may say that this same benign influence has been automatically transmitted to the post-operative period. This great outstanding advan-

tage of local anæsthesia manifests itself not only by its effects upon the surgeon, but, where the method is well standardized, upon all individuals who are brought in professional contact with the patient, before, during and after the operation. The very nature of the demands of local anæsthesia is such that the best interests of the patient must be scrupulously observed if any appreciable degree of success with its use is to accrue.

In considering the patient's comfort alone much may be said in favour of the method. I will take time to mention only the psychic care and the lessening of post-operative complications which follow the ideal exhibition of the method. The results of trauma, lung complications, shock, lowered resistance, with its resulting delayed convalescence and the expense incident thereto must all be taken into account in finally estimating the toll exacted from the patient while undergoing surgical treatment. Any consideration of this subject would necessarily be incomplete without properly evaluating the significance of such points as those just enumerated.

As is the case with general anæsthesia, the successful use of local anæsthesia will depend, to a certain extent, upon the training of the individual who administers it.

Teaching of the method of administering general anæsthesia has been stressed for many years, both in the medical schools and in the best-known clinics, while the development of local anæsthesia methods has been brought about almost entirely through the efforts of individual surgeons and has, until very recently, been frowned upon, or at least neglected, by those who are in a position to mould surgical sentiment. In spite of this condition the success of local anæsthesia has been so great that sentiment in its favour has developed to such an extent that some of the most renowned clinics throughout the world are putting forth most praiseworthy efforts toward its introduction and use. In brief, it is our opinion that local anæsthesia has not gained a stronger foothold, especially in the United States, for some of the reasons referred to above, rather than on account of its shortcomings, although of these it has many.

We are in accord with the position taken by Osler that most men who have reached the age of forty have already

laid the foundation for their life-work. Thus, unless a man has been taught and has practised the use of local anæsthesia during his earlier years, he finds it more or less difficult to revise his methods in conformity with the demands of local anæsthesia.

The writer has, upon former occasions, promulgated this query: "What would be the status of local anæsthesia to-day provided it had received the attention of, and had been employed by, the surgeons who have, for the last twenty years, depended upon general anæsthesia?" The era which confronts us must answer this question.

Speaking from experience, we would predict that such alleged disadvantages as the psychic condition of the patient, adiposity, idiosyncrasy to the drugs used, the protrusion of the intraperitoneal organs, the presence of adhesions, inability to make blind explorations within the abdominal cavity, the division of the surgeon's attention between his work and the patient's psyche, the wasting of the surgeon's time, the necessity for the attention to an extra amount of detail, the demand for special equipment and a host of other points that need not be enumerated here, would no longer be referred to as reasons why local anæsthesia could not be used to advantage.

Factors Governing the Application of Local Anæsthesia.

The successful use of local anæsthesia in major surgery demands, as do all other departures from the orthodox methods, special training and equipment. Without these, its advantages cannot, in a broad sense, be realized.

The successful use of local anæsthesia has in our hands, at least, depended largely upon the following factors: First, team-work, and, second, surgical strategy. As these two factors are so intimately related to each other and to the successful use of local anæsthesia, we shall discuss, briefly, their salient points.

In presenting our ideal in regard to the local anæsthesia method it is the practical application which we wish mainly to emphasize. So-called regional methods, when practical, are most desirable. Similarly the plan of employing an expert to anæsthetize a patient, after which the surgeon walks in and

performs the operation, seems catching and plausible. In cases in which this method is practicable it works out most excellently. The percentage of cases in which this method obtains, however, is too small to become routine. In abdominal surgery it is most absurd, unless one adopts the method advocated by Kappis of using para-vertebral conductive anæsthesia combined with the posterior splanchic method, a method which we believe that the average surgeon will not adopt on account of its danger and the difficulty of learning the technique. We much prefer the direct infiltration, the patient being made comfortable, the field sterilized and draped *before* the induction of the anæsthesia is begun. Every effort is exerted to obtain adequate exposure. All intraperitoneal manipulations are accomplished directly under the eye. The use of gravity, illumination, retraction and deft manipulations and sympathetic anæsthesia by the anterior route induced under *direct vision*, offer a most excellent opportunity for the carrying out of intraperitoneal surgery.

In abdominal surgery the utilization of the force of gravity is most important. The patient should be placed in the desired position as early as possible before operating. If comfortable, the muscles will relax and the viscera will gravitate away from the field which is to be attacked. The anæsthetic should then be administered so subtly that the muscles will not contract during this procedure. Efficient anæsthesia abolishes the muscular reflexes. The proper placing of incisions, the avoidance of pressure while incising, and elastic, symmetrical retraction resulting in a *"negative intra-abdominal pressure"* with an ideal exposure, thus give a most excellent opportunity for surgical attack. Vertical as well as lateral retraction is extremely important.

The development of an ideal exposure serves a twofold purpose. First, it allows one to carry out the necessary surgical procedures directly under the eye. Second, it permits the induction of anæsthesia of the abdominal sympathetics also under direct vision. Exposure, a negative intra-abdominal pressure and deft manipulations, obviate the necessity for the use of traction and abdominal sponges. It also makes it possible for the surgeon to induce intraperitoneal anæsthesia without resorting to the posterior

method of Kappis and in most instances the method of Braun, which is often difficult and somewhat painful, will be found unnecessary. Direct infiltration beneath the posterior parietal peritoneum or into the mesenteries under perfect exposure, is the method we prefer. A negative pressure will, in absence of distention or large growths, usually permit the induction of anæsthesia under direct vision. This method has several advantages. First, its accomplishment demands a perfect technique in carrying out the earlier stages of the operation. Second, it demands the avoidance of traction and the introduction of gauze packs and the most gentle retraction and manipulation. Third, the induction of *anterior sympathetic anæsthesia* under direct vision completely eliminates the possibility of introducing the solution into the blood-stream; and, fourth, it affords the surgeon in many instances an opportunity to attempt the reproduction of the patient's symptoms. This latter we have termed the "Physiologic Test." In our opinion the opportunity to apply this test well-nigh or completely offsets the alleged advantages of the so-called abdominal exploration. Abdominal explorations are not necessarily impossible and may, in many instances, when indicated, be carried out most ideally under local anæsthesia.

BITS OF ANÆSTHETIC WISDOM.

Speaking generally, no anæsthetic is safe.

It is in the *anæsthetist* that safety lies, and not in the anæsthetic or in the apparatus used.

No apparatus, however fool-proof it is said to be, can make the anæsthesia either safe or easy.

Those who "hate giving anæsthetics" . . . . will never become proficient, and will never feel at home.

Those who take kindly to it and like it can never become proficient without experience.

It is not the apparatus that counts, but the man behind it.

Talking should be avoided. Remember that the sense of hearing is the last to disappear.

—Dr. W. E. Hayburn, Bradford, in "The Clinical Journal," July 22, 1925.

LAWSON TAIT, A PIONEER IN CERTAIN METHODS OF ANÆSTHESIA.

Viz: Open ether, warmed ether vapours and E.C. in a Clover's inhaler without a bag.

By W. J. McCardie, B.A., M.B.

AT the Summer Meeting this year of the Section of Anæsthetics of the Royal Society of Medicine in Manchester, Mr. Alexander Wilson, F.R.C.S., Consulting Anæsthetist to the Royal Infirmary, Manchester, told me something of Lawson Tait's work in anæsthesia. Later, he very kindly sent me an abstract of some of it. The subject is so important that I venture to give fuller extracts from Lawson Tait's book, "The Pathology and Treatment of Diseases of the Ovaries" (being the Hastings Essay for 1873), 4th edition, published by Cornish Bros., Birmingham, in 1883.

I quote: ". . . . The agent I have employed for the last six years is free from any objection of an important character. I allude, of course, to sulphuric ether, which is now recognized as by far the safest anæsthetic in use. . . . . The advantages of this agent are very numerous. In the first place, so far as its present use has gone, it is absolutely safe; in my own practice it has been used, on a rough estimate, between five and six thousand times, and not only have I had no accident from it, but its use gives me no anxiety whatever, and whilst I am engaged in operating my mind is absolutely at rest concerning the anæsthetic.* . . . . .

For its proper administration very few rules need be observed and no kind of special apparatus is needed. It is always given for me after the simple fashion which Sir James Simpson introduced for the administration of chloroform, that is by dropping it on the outside of a single fold of a towel, laid upon the patient's face. Bearing in mind that ether is

*Since writing this I have had an accident (on July 15, 1882), due I think to the use of an inhaler. (Ormsby).

extremely volatile, and that its vapour is very heavy, the following directions must be attended to. The towel used must not be too thin, because it must retain a sufficient body of ether for the continuance of the current of vapour; and yet it must not be so thick as to prevent the passage of air freely through it. The ether must be dropped on to the towel, not splashed on, but administered in a continuous stream, which must be allowed to drop from a small orifice on to the towel, above the level of the patient's nose, because the vapour of the ether will fall like a cataract over the patient's face. If the ether is dropped on the towel on a level with the patient's mouth, she will inhale, not the vapour, but a mixture of air and ether, which will act as a stimulant and not as an anæsthetic, and the ether must not be splashed on, for exactly the same reason. The towel should not be tightened over the face, but puffed out around it at a distance of an inch, or an inch and a half, from the skin, in order that it may enclose a body of vapour. The whole of the piece of towel covering the face must be kept continually moist with ether, and in this way a continuous volume of pure ether vapour will be inhaled by the patient. After a few minutes the part of the towel in use must be changed for another part, because anhydrous ether absorbs with intense avidity the moisture of the breath, and the towel will be found coated with ice, and this, by its interference with rapid evaporation, prolongs the process; this is the chief argument against all ether inhalers. Only one other caution need be given, and that is to avoid bringing any light or red-hot cautery near the patient's face while the ether is being given, for it is explosive. . . . . The quantity of ether given during an operation, especially during ovariotomy, is necessarily very large, but no one need ever be alarmed on account of the quantity of ether administered. It is absolutely necessary to keep all the patient's abdominal muscles, except the diaphragm, perfectly quiet; and long before there is the least danger the patient's deep snoring will indicate that the stage of profound sleep has been reached. When the patient snores the administration should for a short time be discontinued. The quiet regularity of breathing which always characterizes the unconscious state induced by breathing the vapour of ether is quite enough to indicate to any one of any

experience in its use that the patient is in a condition of profound insensibility, and this will also serve as the best indication of safety. There is, therefore, very little need for the pulse being watched, or for the conjunctiva to be experimented upon to determine unconsciousness. Should sickness come on during the operation, the ether should be pushed a little more and this will stop it, because sickness really is an indication of returning consciousness. After the operation is over, should sickness occur, it is best checked by the administration of a little lukewarm water flavoured with brandy, and without sugar. When the patient is sick she should always be turned on her side, as in this position the tongue falls forward and the vomit is got easily rid of from the mouth without risk of its entering the trachea."

Warmed Ether Vapour.

To quote again:—"There is only one risk about the administration of ether, and that is the occurrence of bronchitis in old people. I have not seen this often, but in several instances I have had reason for anxiety on this ground, and in order to avoid this risk I have devised an apparatus by which the vapour of ether is given at a temperature of 33° Centigrade, that is, very nearly its boiling point.

When the apparatus is to be used, the tank is filled with water, the spirit-lamp is lighted, and about three drams of ether is pumped into the boiler. Care must be taken that there is no leakage from the boiler, otherwise there will be ignition. It will soon be found that the boiling ether gives over a large volume of vapour at a constant temperature of a few degrees below the boiling-point of the sample, which of course will vary very much, but will generally be found to be pretty nearly the temperature of expired air, 31°—33° C. When given at this temperature, and free from air, the vapour is quite pleasant, and its taste, or rather its comparative freedom from taste, reminded me when I tried it on myself, greatly of the flavour of nitrous oxide. It is, of course, certain that its administration will involve no risk of bronchitis.

I think this method will be found to be a substantial advance in the method of ether administration for old people; and if care be taken to keep the boiler vapour-tight, and the

mouth-piece three or four feet away from the spirit flame, I think the apparatus will be found quite safe."

E_2C_1 in a Clover's Inhaler without the Bag.

In 1898, when I was collecting materials for a paper* "On the administration of a certain mixture of chloroform and ether in gynæcological operations by means of a Clover's inhaler without the bag," Mr. Lawson Tait wrote me as follows:—"Then (that is about the year 1882) we found a mixture do well—2 ether and 1 chloroform—and that, I believe, is the best of all anæsthetics, save in old age and damaged kidneys, for all kinds of work. I think, if introduced by nitrous oxide, it would be an ideal anæsthetic."

This method of the administration of E_2C_1 by means of a Clover's regulating inhaler has always been known in Birmingham as Lawson Tait's.

For the purpose of this paper I had collected statistics of fourteen thousand odd cases in hospital and private work in which this method was used. In the series there were three deaths, two of which occurred in women who were practically in extremis, and the other in a very healthy middle-aged woman.

In the Women's Hospital, Birmingham, about 95 per cent. of all operations were performed under this anæsthetic. Mr. Christopher Martin, F.R.C.S., who was Mr. Lawson Tait's assistant and later partner, tells me in his time, that is from 1889 onwards, for many years all Mr. Tait's private, as well as hospital patients, and those of the other gynæcologists, were thus anæsthetized.

Lawson Tait's method is still used in Birmingham and has undoubtedly proved its value in very many thousands of gynæcological cases before and after 1898. Thus we see that besides his original operative work in the surgery of the gall-bladder, in extra-uterine fœtation and in the repair of ruptured perineum, etc., Lawson Tait also did pioneer work in anæsthesia.

*_Lancet_, December 17th, 1898.

NITROUS-OXIDE - ACETYLENE - OXYGEN ANÆSTHESIA FOR EXTRACTION AND ORAL SURGERY.

By

AARON GOLDMAN, D.D.S., and JULIUS D. GOLDMAN, D.D.S.
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(Presented before the Fourth Annual Congress of Anæsthetists, at Atlantic City, May 25—28, 1925.)

NITROGEN ANÆSTHESIA.

FOR short anæsthesias nitrogen alone or with atmospheric air or with small percentages of oxygen may be used. The unconsciousness and insensibility to pain will last for a short period. "The anæsthesia must be regarded as the result of oxygen-deprivation, as it is difficult to assign any specific anæsthetic properties to nitrogen itself."[1] However, were there no better anæsthetics available, it is probable that a technique would be developed for the use of nitrogen with oxygen for anæsthesia. Nitrogen undergoes no chemical combination with any of the tissues of the body. The spectrum of hæmoglobin saturated with nitrogen is the same as of hæmoglobin alone.

NITROUS-OXIDE ANÆSTHESIA.

In the files for the year 1848, of a New England Medical Journal, the curious may come across a report of the removal of a tumour, in which nitrous-oxide anæsthesia up to the point of asphyxia was applied for fifteen minutes, during which time the mask was applied, removed, and re-applied a number of times. Many similar reports are scattered through medical journals.

Scientists are not in accord as to the action of nitrous-oxide. Many have claimed that it has definite anæsthetic effects. Others say that it acts merely through the reduction of oxygen in the body, that is, definitely as an asphyxiant.

Nitrous-Oxide-Oxygen Anæsthesia.

In 1868, Andrews of Chicago administered nitrous-oxide with oxygen for prolonged anæsthesia. About a decade later Paul Bert, one of the world's most brilliant investigators, reported to the Academy of Sciences, Paris, that nitrous-oxide with oxygen will not anæsthetize.[2] And many German and French writers have quoted Paul Bert during the last five years, overlooking the fact that nitrous-oxide with oxygen has been extensively administered for over thirty years in the United States especially.

Technique.

To help the reader develop a technique which will be best in his hands we will present Dr. McKesson's, Dr. Heidbrink's, and then our own. As we have tried in our not narrow experience various methods, we consider our own the best—*in our hands.*

Heidbrink's Technique.

Set oxygen 5 per cent. and nitrous-oxide 95 per cent. Apply nasal inhaler and mouth cover. Open stop-cocks to both. Patient then may breathe the gases through the mouth as well as the nose. Instruct patient to interlock fingers, place hands in the lap, breathe naturally through the nose and go to sleep.

At expiration of one minute administer nitrous-oxide alone, until anæsthesia is complete (usually 30–60 seconds). Normal adults will reach the operative stage of anæsthesia approximately 40 seconds after the oxygen has been discontinued. Rhythmic breathing and the disappearance of eyelid reflex indicate complete anæsthesia.

McKesson's Secondary Saturation.

Dr. B. H. Harms of Omaha, in a splendid article, presents three methods to maintain a smooth, quiet anæsthesia with nitrous-oxide and oxygen. The first two, (1) pre-anæsthetic medication and (2) synergistic anæsthesia, he discards; and recommends (3) the McKesson method of secondary saturation. Nitrous-oxide alone is given until signs of anæsthesia appear, then add a small percentage of oxygen until the stage of profound anæsthesia, which is noted by all signs of an

overdose, as prolonged expiration; phonation due to muscular spasm of vocal cords; wild looking expression; tetanic spasm, marked rigidity, opisthotonus in some cases; cyanosis usually, etc. This profound stage is maintained only momentarily to secure complete or secondary saturation with nitrous-oxide. At this point it is necessary to give the patient two or three breaths of oxygen, or if breathing has stopped pure oxygen must be forced into the lungs under pressure. For major dental surgery normal anæsthesia is required, but it is often necessary to invade the profound plane momentarily in order to attain normal anæsthesia.

Our Technique.

Our technique differs from those outlined above. For *prolonged* anæsthesia with nitrous-oxide and oxygen we believe in a long period of induction. This obtains the necessary saturation with easeful accommodation of the tissues to the new conditions. In a way we may compare this to the slow injection of novocain in conduction anæsthesia. The bag is filled with nitrous-oxide oxygen ::92 per cent. and 8 per cent. The patient is instructed to breathe normally, easily, through the nose and go to sleep. The mouth prop is inserted. The face mask is closely adapted. After four or five inhalations of atmospheric air the valve box on the inhaler is closed and the patient commences to breathe the gases from the bag. The valves are turned on, gradually decreasing the oxygen below 8 per cent. During the slow induction stage the concentration suitable for the patient is determined. The face inhaler is removed. The nasal inhaler is applied. The pharyngeal pack is carefully inserted. Commence the operation if signs of good, normal anæsthesia are present—regular, machine-like breathing, slightly more rapid than normal; colour slightly dusky.

With an induction during which the requirements of the patient have been established, an even anæsthesia can generally be maintained with little variation of the concentration.

Nitrous-Oxide an "Indifferent" Gas.

Nitrous-oxide undergoes no chemical combination with any of the tissues of the body. Spectroscopic examination of

the blood during anæsthesia has revealed nothing definite in new hæmogoblin combinations. The spectrum of hæmoglobin saturated with nitrous-oxide is the same as of hæmoglobin alone. Purists in language may object to the terminology, but it is on the above grounds that we speak of nitrous-oxide as an " indifferent " gas. (Peyton Rous, of the Rockefeller Institute, is studying calorometric changes in functioning organs under different anæsthetics. Wieland, as others before him, speaks of a " Winterschlaf " during anæsthesia.

Reduced Intra-Cellular Oxidation.

For years many of us have been in search of another " indifferent " gas but with which sufficient oxygen could be used to maintain the normal oxygen content of the blood. In nitrous-oxide oxygen anæsthesia the oxygen content of the blood is markedly reduced.

" Because of the action of nitrous-oxide on the intracellular oxidation, its use is fraught with danger." (Crile.)

" Anoxic-anoxæmia is always present." (Green.)

Ethylene-Oxygen Anæsthesia.

The findings of Brown, Luckhardt, and Carter elated us. It was shown that much higher percentages of oxygen could be used with ethylene for anæsthesia than with nitrous-oxide. It was reported that 20 per cent. oxygen—80 per cent. ethylene would produce anæsthesia. Our clinical and laboratory findings did not agree with these reports. As the enthusiastic users of ethylene appeared to agree amongst themselves, and as our figures disagreed with them, we secured an apparatus, different from the one we had been using, and similar to the one that was in common use in the hospitals from which the reports on clinical and laboratory findings with ethylene were being issued. We soon found the source of their error. It had been taken for granted that the flow of nitrous-oxide and of ethylene was the same. The nitrous-oxide gauges had not been corrected for ethylene. Until lately ours were the only gauges especially calibrated for ethylene and acetylene.

Ethylene-Oxygen Technique.

Our technique for ethylene and oxygen is the same as for

nitrous-oxide and oxygen, except that more oxygen is administered; on the average probably 7 to 12 per cent. The induction period is, in general, shorter. The patient may appear one or two strata lighter, and the same operative procedures may be performed as would require the deeper strata with nitrous-oxide and oxygen. There is less need of a vapour synergist although a small amount of the vapour synergist is quite an advantage. Vomiting and retching, although brief, are more prevalent than with nitrous-oxide and oxygen.

Cyanosis ?

A few years ago, in speaking on nitrous-oxide and oxygen anæsthesia, we said that cyanosis should never be permitted for any length of time. During maintenance it may appear momentarily but should never be continued. With animals we have never been able to get and maintain nitrous-oxide and oxygen anæsthesia without cyanosis. Is it possible with the human? Evidently not, if we are to give the word the broader meaning of reduced blood aeration. The Greek root, however, means dark blue—thus we speak of certain cyanogen compounds which are deep blue. In this latter sense neither is the animal nor the human kept cyanotic except momentarily. Let us say rather that they cannot be kept pink. As from 7 to 12 per cent. oxygen is permitted with ethylene, neither can the animal nor human be kept pink with ethylene and oxygen. If we add a vapour synergist to the gases increasing the oxygen content, pinkness may be maintained.

The Goal Unattained.

To repeat, when in 1922, and 1923, Brown, Luckhardt, and Carter presented their findings we were all overjoyed. An important step forward had been made. But further laboratory study and clinical experience soon showed that the much-desired goal had not yet been reached. Much less than the percentage of oxygen in the air can be used with ethylene.

Acetylene-Oxygen Anæsthesia.

Acetylene-oxygen meets the demands more fully. Our research founded upon the research and clinical experience of our German confrères, and our clinical experiences since

December 1924, in the use of nitrous-oxide-acetylene-oxygen for extraction and oral surgery prove that a long step forward has been made in the search for an ideal general anæsthetic. With nitrous-oxide-acetylene-oxygen the oxygen content of the blood is normal throughout the anæsthesia.

When we commenced to employ acetylene-oxygen no one had yet used this anæsthetic for extraction of teeth or for oral surgery. Mr. Coltman, the chemist who had helped so much in purifying the gas, holds the distinction of being the first to take the gas for this purpose (the removal of two teeth and of an area of rarefaction about them). The anæsthesia was maintained 5½ minutes, oxygen gradually increased from 25 to 45 per cent. (October 1924.)

Lately one of us took the gas for the removal of infected adenoids, Dr. Gwathmey and the other of us giving the anæsthetic. The anæsthesia was maintained for 12 minutes during which the oxygen was gradually increased from 20 to 53 per cent. In both cases the patient awoke smiling—no retching and no vomiting. In Mr. Coltman's case a pharyngeal pack was used. In the latter case there was an ingress of air by way of the mouth throughout the operation—but this did not interfere with the anæsthesia. The patient remembers the first seven breaths through the nose-piece. From then until he was moved to a stretcher to be wheeled to the elevator he remembered nothing. Face and cheeks ruddy throughout the operation.

Hermann Wieland, in the January 1925, issue of the *Brit. Journ. of Anæsthesia,* writes: "Up to the present this mode has been employed only in general surgical and gynæcological operations. Experience as regards suitability for operations in ear, nose, and throat, as well as teeth, is wanting." This want we have commenced to fill. At the beginning of 1924, we commenced the use of ethylene. The anæsthesia would be commenced with nitrous-oxide and oxygen to disguise the odour, then we would either add ethylene to the other two gases or substitute ethylene for nitrous-oxide. Except that most writers were unintentionally in error in regards to the percentages, our findings agreed with theirs. Ethylene is superior to nitrous-oxide. At the end of 1924, we substituted acetylene for it. The contrast between acetylene and ethylene

is greater than that between ethylene and nitrous-oxide. *Ethylene and acetylene have exactly the same disadvantages. Acetylene has a most important added advantage.*

NITROUS-OXIDE-ACETYLENE-OXYGEN TECHNIQUE.

The nasal inhaler is adapted and the mouth prop inserted. To disguise the odour we commence with nitrous-oxide and oxygen. After about 8 to 15 inhalations we shut off the nitrous-oxide and administer acetylene-oxygen ::80:20. The patient loses consciousness in one minute. The oxygen is increased to 27 per cent. The towel over the mouth is removed. The pharyngeal pack is carefully inserted and the mouth prop is sometimes readjusted. The roots to be removed and the area are carefully examined with the aid of a pair of college pliers or an elevator. If the operator has passed through the school of experience leading from—

(1) Nitrous-oxide alone; to
(2) Nitrous-oxide with oxygen in short periods of induction; to
(3) Nitrous-oxide and oxygen with longer periods of induction; to
(4) Nitrous-oxide and oxygen with vapour synergists; to
(5) Ethylene and oxygen alone and (*b*) with vapour synergists;

if he has passed through this school of experience he has learned to do his work carefully and well and not in haste. A few pages back in speaking of ethylene-oxygen we said: "The patient may appear one or two strata lighter and the same operative procedures may be performed with as thorough a brain block as would require the deeper strata with nitrous-oxide and oxygen." With acetylene and oxygen the patient appears to be one or two strata still lighter than with ethylene-oxygen and the same surgical procedures may be carried out with a total brain block. During the maintenance stage we raise the oxygen content gradually every two or three minutes to a concentration which appears to be best for the given case. Should the patient show signs of waking we decrease the oxygen to 27 per cent. for one minute, and then gradually increase the oxygen content again to a point below the oxygen

content that was reached at the time the waking appeared to occur. As a rule the longer the anæsthesia the higher the oxygen content. Patient awakes in one to three minutes.

Gas-Oxygen Apparatus.

Nitrous-oxide, ethylene, acetylene, carbon dioxide, oxygen are stored in tanks under high pressure. These gases are to be delivered to the patient in desired volume and concentration under ordinary or slightly increased atmospheric pressure. On the way to the inhaler they may be made to take up a vapour synergist.

There are many good machines for gas anæsthesia on the market. All are simple. Their complexity is only apparent. The novice will master the mechanism in a shorter time than it would take him to learn to run a Ford.

The function of a gas apparatus is to deliver to the patient gases under low pressure from tanks containing the gases under high pressure; and to permit an easy change in the volumes and the proportions of the gases.

Lung Respiration.

The piping and the breathing bag are an extension of the patient's lungs. The gases pass into the lungs and mix with the reserve and residual air in the alveoli of the lungs. A luxurious capillary network studs each air cell. The thinnest of partitions separates the gases of the lungs from the blood supply—the single layer of flattened cells of the air cells and the single layer of thin endothelium of the capillaries. The surface over which the gases are almost in contact with the blood of the capillaries is over 100 times the surface of the patient's body. The venous blood of the pulmonary arteries absorbs the gases from the air cells. The blood stream passes to the heart by way of the pulmonary veins and is coursed through the body. The process is the same whether nitrogen and oxygen, or nitrous-oxide, acetylene and oxygen are inspired.

Apparatus for more than Two Gases.

The apparatus for acetylene and oxygen will not differ from that for nitrous-oxide and oxygen, only the gauges will

be re-calibrated. An apparatus for three or four gases will have three or four intake tubes instead of two.

What do we require of an apparatus?

(1) To give us, with ease, a regular flow of the gases in the volumes and the proportions we want them.

(2) With ease, and at a glance, both the operator and the anæsthetist should be able to determine the proportions and the volumes of the gases delivered.

(3) In the course of the gases there should be a container for adding to the gases whatever vapour (anesthol, ether, etc.) we may desire.

(*To be continued.*)

NOTES.

Prof. Dr. Hermann Wieland, who contributed an important paper, "Acetyline Gas as an Anæsthetic," in a recent number of this Journal, has taken over the Directorship of the Institute of Pharmacology, Heidelberg.

Dr. Z. Mennell, London, has been elected Executive Officer for England, by the Canadian Society of Anæsthetists.

At the recent Congress of Anæsthetists, Atlantic City, N.Y., a committee was appointed to devise ways and means of sending Dr. and Mrs. F. H. McMechan to Europe in 1926 to further International relations in the speciality of anæsthesia.

Dr. John J. Buettner, in his Presidential Address at the 1925, meeting of the Canadian Society of Anæsthetists, made the excellent suggestion that memorial lectures commemorating the lives and works of the pioneers in anæsthesia be a feature of future annual meetings.

The following officers have been elected for 1925—1926, Canadian Society of Anæsthetists: President, Chas. La Rocque; Vice-President, Chas. H. Bastin; Secretary and Treasurer, Harry J. Shields; Executive Officer for Great Britain, Z. Mennel, London.

The Associated Anæsthetists elected John H. Evans, President; Gervaise E. Johnson, 1st Vice-President; Wm. W. Hutchinson, 2nd Vice-President; Henry F. Becker, 3rd Vice-President; F. H. McMechan Secretary-General.

The International Anæsthesia Research Society: John H. Evans, Chairman; E. Klaus, Vice-Chairman; F. H. McMechan, Editor-Executive Secretary.

Scottish Society of Anæsthetists, 1925—1926.—President, A. Mills; Vice-President, J. Stuart Ross; Hon. Sec. and Treas., Fred L. Napier; Executive: Aberdeen, Ross Mackenzie; Dundee, A. Mills; Edinburgh, H. Torrance Thomson; Glasgow, Wm. Barras.

Correspondence.

A CONGRESS OF ANÆSTHETISTS.

(To the Editor.)

Dear Sir,

Both at the Toronto Meeting and the Congress the Associated Anæsthetists voted to send Mrs. McMechan and myself abroad next Summer to extend contracts among the anæsthetists on your side of the Atlantic. I am writing you in the hope that the British anæsthetists would undertake the organization of a Congress of Anæsthetists in connexion with the meeting of the British Medical Association next year.

In that event we would abandon our Congress of Anæsthetists on this side for 1926, and make every effort to induce as many American and Canadian anæsthetists as could be persuaded to go abroad with us for the meeting in England. This would enable us to make somewhat the same tour as the recent visit of the Inter-State Post-Graduate Clinic Society.

I rather imagine that quite a few prominent research workers and surgeons from the Continent, interested in the latest advances in anæsthesia, might be attracted to such a Congress in England. We would, of course, endeavour to secure our best research workers and anæsthetists to take part in the programme. I believe almost a full year would give us ample time to work out the necessary details for a joint Congress, and we might even attract some Anzac anæsthetists.

I should like to know how this idea appeals to our colleagues in Great Britain. If it meets with a favourable reception on all sides you can rest assured of our entire co-operation and support.

Yours, etc.,

F. H. McMechan,
Secretary-General, Associated Anæsthetists
of the United States and Canada.

Avon Lake, Ohio,
August 19, 1925.

GAS AND SUDDEN COLLAPSE.

(To the Editor).

Dear Sir,—

I should be very much obliged if one or more of your readers would give me the real explanation of why one occasionally meets with cases of sudden collapse after the administration of Gas, or Gas and Oxygen for Dental extractions. I have had two such cases during the last year, and they have given rise to a good deal of anxiety, and, to be perfectly candid, I am at a loss to understand the reason why they did collapse.

The first case was a man of about forty-five, about 6 ft. 2 in. in height, spare of build and looking septic. He was anæsthetized with Gas and Oxygen with a Hewitt's machine, and the anæsthesia continued with Gas by an ordinary nose-piece. The anæsthesia was perfectly normal; he came round and was rinsing his mouth with water, when he exclaimed, "Oh! I do feel rotten!" Almost before I could get to him he had fainted. His pupils were widely dilated, he was pale and cold. I thought for a moment that he was dead, but I rapidly depressed his head down to his knees and in a few moments he became conscious again and sweated profusely. His pulse was very feeble, but after the administration of a little brandy he recovered somewhat. He was not, however, fit to leave the dentist's room for nearly an hour. He volunteered the statement that he had never fainted before.

The other case was a man of similar build and of extremely nervous disposition. Here the anæsthetic was a very short and simple affair, and about five minutes afterwards the patient said he was feeling very ill. He then fainted, but, in contra-distinction to the other case, his pupils remained contracted all the time. His own doctor was present, and he administered, firstly, strychnine, and then later caffeine, and as neither of these injections seemed to have much effect, we then gave him a little brandy by the mouth, and as he was a life-long teetotaller this seemed to give a good result. In this case it was about ten minutes before the patient told us that he could see! Here again it was a full hour before he was fit enough to be sent home with his doctor.

The question I should like to ask is. "Why do these cases occur, and what is the cause of it?" I shall be very grateful to any of your readers who can give me a really clear and satisfactory explanation.

I am led to write this letter, because a week or so ago I saw in the lay Press that a man had died *after* the extraction of teeth with Gas. He had collapsed and died. That was the brief statement, but it leaves us no wiser.

In an experience of nearly twenty-five years I have only had these two cases that I now report, and I cannot but think that it would be of advantage to all of us if we could get the views and experience of some of the other anæsthetists on this topic.

Yours faithfully,

H. E. G. Boyle.

RETCHING AND VOMITING.

Dear Sir, (*To the Editor.*)

We would like to get some information as to retching and vomiting—during and following anæsthesia and from other causes. Do you know of any studies that have been made?

From our experience two interesting items are worthy of mention:—

(1) In over 1,000 anæsthesias with $N_2O-C_2H_2-O_2$ we have never experienced retching or vomiting *during* an anæsthesia. With N_2O and O_2 or $N_2O-C_2H_4-O_2$, although not frequent, retching and vomiting did occur.

(2) In the last 16 cases, eight of whom had eaten a meal within two hours before taking the anæsthetic, there was no retching or vomiting after the anæsthesia. The technique was the same as that described in our paper except that the induction was commenced with N_2O alone, eight to fifteen breaths (no O_2). Period of anæsthesia three to sixteen minutes.

Thank you, in advance, for whatever help you can give us.

Sincerely,

A. & J. D. Goldman,
63 W. 48th Street, New York City.

Dr. I. M. Rabinowitch, Director of the Department of Metabolism at the Montreal General Hospital, who contributed an important article in our July Number, has been appointed an Assistant Professor in the Faculty of Medicine, McGill University.

PROCEEDINGS OF SOCIETIES.

SCOTTISH SOCIETY OF ANÆSTHETISTS.

(Continued from July No. 47.)

Report of the Proceedings of the Annual Meeting held on 9th May, 1925, in Dundee.

The above meeting was held at the University College, Dundee.

Dr. J. S. Ross read a paper on the subject of

BLOOD PRESSURE UNDER NASAL GAS.

The qualities which render an anæsthetic useful to the dental surgeon are briefly three in number. Firstly, it must be essentially safe if given with reasonable skill. Secondly, it must cause a minimum of after-effects in the patient. Thirdly, it must give an anæsthesia of sufficient duration to permit of the work in hand being performed without undue hurry. In Edinburgh the method described by Dr. Guy and the present writer in numerous papers, of nitrous oxide and oxygen with small doses of ethyl chloride, has for some years been very extensively used. In addition to its safety, it has the virtue of being very simple to use; with it the dentist feels that he needs the help of a skilled expert only on rare occasions. It is, however, a " single dose " anæsthetic and has the faults of all such. The duration of anæsthesia is always sharply limited, ninety to a hundred seconds being about the outside, and in certain cases (impossible always to identify beforehand) the period may be even shorter. If recovery begins before the work is finished, the operation must be stopped or the patient suffers pain. This is a most annoying dilemma and a method in which it is a fairly frequent feature can never be accepted as the last word on the subject. Nonetheless, it does suffice for the larger number of extraction cases, particularly those met with in good class private practice, and the present writer does not wish to withdraw anything he has said in the past in its favour. There remain, however, a certain number of cases where more time is necessary and a still greater number in which more time would be a real help. If the operator has no fear of the patient coming out before the tooth, he will be much less likely to break or miss a root. Nasal methods must therefore always have a scope.

To an audience such as this I need not dilate upon the physics of nasal administrations. We all now know that the older instruments failed chiefly because they supplied no expiratory valve on the nose-piece, and that no apparatus which does not possess this fitting is worth much attention. It is only a small proportion of people who can be induced to breathe quietly in by the nose and out by the mouth, while most can bring themselves to breathe up and down the

nose even with the mouth well opened. Given this essential fitting, there is no doubt a wide choice of suitable apparatus, but it would probably be hard to improve on the Coleman Aseptic Inhaler, which is the plant with which the writer is personally acquainted. The really moot point is, whether the addition of oxygen to the nitrous oxide is essential. Beyond doubt such addition involves added bulk and weight to the plant though I hardly think the increase of complexity is a serious matter. Coleman has, however, pointed out that once the operation has begun, air will almost certainly enter through the mouth with some freedom, and in a proportion quite sufficient to prevent serious anoxæmia, and to judge by the clinical signs of the colour, the size of the pupil, and the type of respiration, one feels that he is probably right in the great majority of cases. Definite confirmation on the point by a method which showed some approximation to scientific accuracy did, however, appear to the writer desirable, and the following observations were made in an attempt to obtain it.

In addition to the clinical signs of anoxæmia above referred to, the most striking feature of oxygen starvation is a rise of systemic blood pressure. Since such rise occurs early, and before serious danger has been induced in a healthy subject, and since it is easily capable of measurement, this sign was chosen as the measure of the anoxæmia. In all 32 cases were recorded. Blood pressure readings were obtained immediately before beginning the gas, and also throughout its administration at frequent intervals, the time being recorded in seconds. It should be made clear that the administration was in no sense *guided* by the readings obtained. The writer acted as anæsthetist, and endeavoured to close his ears to the remarks of those who were attending to the sphygmomanometer. He was guided purely by the usual clinical signs of colour, pupil and breathing, and aimed at securing an anæsthesia just adequate to the work in hand without reference to the results being obtained by the other members of the team. The blood pressure changes may therefore be taken as representing those which must be occurring in the practical use of the method when given in one's daily work.

Taking first the changes in pressure recorded in the period of induction, it came as a gratifying surprise that in no less than 23 out of the 32 cases, the rise up to the moment when the operation began, did not exceed 15 mm. of Hg. Such a change of pressure in the arteries must of itself be of little if any importance, and it would appear that in this high percentage of cases we can obtain good working anæsthesia without any serious anoxæmia. Of the remaining nine cases, five showed a rise of 15 to 20 mm. Hg, two of 21 to 25 mm., and two of 26 to 30. In none of these did the cyanosis strike one as at all serious, but one is bound to admit that the anæsthesia in a few cases must have been unnecessarily deep, and that a good result could have been obtained without going quite so deep.

The work had not gone very far before one became aware of a point which had not been foreseen, though probably it ought to have been. Quite a number of cases showed during the operation pressure decidedly higher than they did even at the very end of the induction period. In every case the mouth cap was used up to the moment of beginning the operation, while during its performance the mouth cap was of course not in use and a varying amount of air would necessarily be inhaled per orem. In some cases this would of itself not be sufficient to keep the colour good, but in that event the administrator, guided as he was purely by clinical signs and not by the blood pressure readings, remedied matters by opening the air-port of the apparatus from time to time. Exceptionally it would be found necessary to deepen the anæsthesia after the operation had started, but if this were done a note was made by the administrator *before he had seen the blood pressure reading*. Any post-induction rise of blood pressure would in such a case be ascribed to the increased anoxæmia associated with the deepened anæsthesia, but in the other cases a rise of pressure after the operation had begun should, I think, be attributed to some cause other than increase of oxygen starvation. Such a factor can only be found in the reflex effects of the trauma necessarily inflicted by the operator, and one can hardly doubt that this is the true explanation of the phenomenon. This post-induction rise was observed in eleven cases. Eight cases showed a rise of 5 to 15 mm. Hg, compared with the reading immediately before the operation began; two cases showed a rise of 20 mm., while in one case it was as much as 30 mm. Putting the matter in another way, there was during the induction stage a rise of more than 15 mm. Hg. in nine cases only, while during the operation 18 cases exceeded the 15 mm. line. These figures can hardly be explained as the result of accident or experimental error, and to the writer's mind prove fairly conclusively the reflex element in the readings. The degree of trauma is hard to estimate in any individual case of tooth extraction. Certainly the number of teeth removed is no measure of it. But so far as we could ascertain, it was in the cases where considerable trauma was necessarily inflicted that the rises were most marked in the post-induction stage.

Let me give a few examples.

Case No. 25. Female, aged 28. Two fairly tight teeth to be extracted. Initial blood pressure 120 mm. Duration of induction 80 secs. Blood pressure at end of induction 120 mm. (*i.e.*, no rise). Duration of operation 30 secs. Maximum blood pressure during operation 140 mm. (three readings during operation all gave the same figure).

Case No. 24. Female, aged 17. Six teeth, fairly tight. Initial blood pressure 140 mm. Duration of induction 50 secs. Blood pressure at end of induction 150 mm. Duration of operation 40 secs.

Maximum blood pressure during operation 180 mm. This figure seems very high; the readings during each of the four ten-second periods were successively 150 mm., 160 mm., 160 mm., 180 mm. In forty seconds the blood was back to the patient's normal of 140 mm. The anæsthesia was on the light side, slight phonation being noted during the operation.

Compare with these Case No. 17, where the trauma was not sufficient to cause any rise during operation.

Case No. 17. Male, aged 18. One tooth only. Initial blood pressure 125 mm. Duration of induction 60 secs. Blood pressure at end of induction 144 mm. Duration of operation 30 secs. Maximum blood pressure during operation 140 mm. There was here a point of some interest. The anæsthesia was quite deep and the patient showed no reflex movement. During the recovery stage, however, he evidently had disturbing dreams and began to strain and shout, thereby running his pressure up to 160 mm. (a rise of 20 mm.) after all anæsthetic had been withdrawn.

This case drew one's attention to the factor of breath-holding as a possible source of post-induction rises. Some interference with the regularity of respiration must be a common feature of all short anæsthetics, particularly those given for dental purposes, however skilful the administration; but careful observation subsequent to this case did not give one the impression that it could account for many of the cases where post-induction rises were recorded.

Let me refer to one or two points about nasal gas apart from blood pressure effects. The average time necessary to induce anæsthesia was 78 secs., the minimum being 50 secs. and the maximum 150 secs. I think this merely means that there is often great difficulty in excluding all air from the respiratory flow. When one is accustomed to work with the ordinary face-mask, this delay when inducing with a nasal mask is a little disconcerting, but one gets accustomed to the extra time, and probably to the patient it makes little if any difference.

In taking the observations we were unfortunate in the length of time required for the operation, the average duration of which in these cases was only 64 secs. I would have been quite glad if the extractors had been rather less expert. The longest case we had took 60 secs. to induce while the operation lasted 210 secs. The result was excellent in all save one respect. [illegible] arm upon which our appliance was fixed was held rigid throughout the whole period and gave the team no chance to get any readings after the operation had begun. This was really rather hard luck.

As time was limited discussion on Dr. Ross's paper was postponed until the next meeting.

In the evening the members dined together.

FRED. L. NAPIER,
Hon. Secretary.

SOME ABSTRACTS FROM AN OLD BOOK.

Seventy-nine years ago, or to be more exact, on the 16th day of October, 1846, Morton gave his first demonstration of the anæsthetic value of ether. The discovery was hailed with the greatest enthusiasm, and in a very short time ether was being used in many parts of the world. But, inconceivable as it may appear to us at this time, there was a period when men carried on a terrific campaign against it; and the discoverer was compelled to fight for its very life against almost overwhelming odds. In reading over the account of this very bitter controversy in "The Trials of a Public Benefactor," a work by Nathan P. Rice, published in 1858, just about the time when the fight was at its height, and knowing as we do to-day the incalculable value of Morton's discovery, it is indeed difficult for us to realize that "for years he had been the mark for the shafts of calumny, falsehood and envy."

Here are a few extracts from statements published at the time:—

Prof. A. Westcott, Baltimore: "If Dr. Morton can make *me* believe that the indiscriminate application of this vapour is really so very harmless, he will make me believe that I am the richest man on earth. For who would not administer a harmless thing, especially when it is endorsed by the first surgeons and medical men of New England, to stop the crying of cross babies. If the thing is really harmless, and the subjects, 'after breathing it from one half to two minutes, drop into a *quiet slumber*,' it certainly would be invaluable for nursery purposes. *Morton's sucking bottles* would be in great requisition, surely."

The Philadelphia *Medical Examiner*: "We should not consider it entitled to the least notice, but that we perceive, by the *Boston Medical and Surgical Journal*, that prominent members of the profession in that city have been caught in its meshes. We are persuaded that the surgeons of Philadelphia will not be seduced from the high professional path of duty, into the quagmire of quackery, by this Will-o'-the-wisp. We cannot close these remarks without again expressing our deep mortification and regret, that the eminent men who have so long adorned the profession in Boston, should have consented for a moment to set so bad an example to their younger brethren, as we conceive them to have done in this instance. If such things are to be sanctioned by the profession, there is little need of reform conventions, or any other efforts to elevate the professional character: physicians and quacks will soon constitute one fraternity."

The New York Annalist: "By-and-by we may see 'Morton's Antipathetic Inhalation' puffed in an article to which shall be appended the honoured names of Warren, Bigelow, and Pierson; and wherein, we ask, will it differ from the objectionable contributions of

others, quite so high, to Swain's Panacea? . . . . The last special wonder has already arrived at the natural term of its existence; and the interest created by its first advent has, in a great measure, subsided. It has descended to the bottom of that great abyss which has already engulfed so many of its predecessor novelties, but which continues, alas! to gape, until a humbug yet more prime shall be thrown into it."

New York *Journal of Medicine*: "We are sorry to see many of our brethren, at home and abroad, stooping from the exalted position they occupy in the profession to hold intercourse with, and become the abettors of, quackery in any form. Such doings are certainly contrary to the ethics of the profession, and should not be tolerated for a moment in anyone."

These extracts are but a few of the many articles that were appearing regularly in current medical periodicals demonstrating the animus as well as incredulity which Morton had to overcome. Indeed he found it necessary to issue a circular, printed almost every week at his own expense, in which he replied to these attacks as best he could.

Strange as it may seem, although the land of its birth at this time proved such a hostile ground for the propagation of the new discovery, Europe, from the very beginning, grasped the value of, and put into practice, ether anæsthesia.

It is especially interesting to quote from English periodicals of the day:—

People's London Journal, Jan. 9, 1847: "Good News from America. Hail, happy hour! that brings the glad tidings of another glorious victory. Oh, what delight for every feeling heart to find the new year ushered in with the announcement of this noble discovery of the power to still the sense of pain, and veil the eye and memory from all the horrors of an operation. And then to find it acted upon almost on the instant by our first operators, is as gratifying as unexpected. *We have conquered pain.* This is, indeed, a glorious victory to announce; a victory of pure intellect . . . . It is a victory not for to-day, nor for our own time, but for another age, and all time; not for one nation, but for all nations, from generation to generation, as long as the world shall last. Let the joyous news spread quickly from ear to ear through all the length and breadth of the land, and wing its way over the seas from shore to shore. And you, poor sufferers, who are now lying in our hospitals and infirmaries on the bed of sickness, waiting your time for the dreaded operation, hear you the reprieve which has been sent! Fear no more the pain that you shall endure—a sweet oblivion shall steal over your nerves."

The Lancet: "Dr. Morton deserves, if his discovery stands the test of time, the gratitude of every civilized people and government upon the face of the earth."

ABSTRACTS OF CURRENT ANÆSTHETIC LITERATURE.

The alleged synergism of magnesium sulphate and morphin. Harry Beckman. *Jl. Amer. Med. Assoc.*, Chicago, Aug. 1, 1925.

Dr. Beckman asserts that all writers on this topic interpret their findings on (1) a misunderstanding, and (2) false conception. The misunderstanding is due, he avers, to entirely inadequate, though sincere, animal and human experimental work, while thorough scientific studies prove quite the contrary.

The misconception consists in ascribing to synergism the overwhelming effect seen when abnormally large doses of either drug is given, *i.e.*, it is necessary to distinguish clearly between addition and synergism.

He questions the value of experiments in labour with magnesium sulphate and morphia in combination when tests were not made of the effects of morphia alone. He also thinks the dosage of morphia was somewhat heroic in itself in the combination, and in the few cases reported where morphia alone was depended upon the drug was not given in the same large amounts in which it appears when the combination with magnesium sulphate was employed.

Dr. Beckman utilized 51 dogs in his 113 experiments, his object being (1) to determine exactly the conditions of successful colonic anæsthesia in the dog when morphine alone was used as the preliminary medication; and (2) to observe to what extent, if any, these conditions could be modified by the use of magnesium sulphate plus morphine as the preliminary medication.

He asserts that these experiments prove that the alleged synergism between magnesium sulphate and morphine does not take place in dogs.

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116 *cases of sacral, and 26 cases of spinal anæsthesia, in urologic operations.* Frederick J. Parmenter. *Bulletin*, Buffalo General Hospital, April, 1925.

In 105 sacral cases the anæsthesia proved successful in 77, imperfect in seven, and unsuccessful in 18 cases. Severe reactions occurred in five, very severe in four, and in two cases bad as ever seen in spinal anæsthesia.

In the series of 26 spinal cases anæsthesia was perfect in 18, and in five gas was necessary. In three, anæsthesia failed completely. As to reactions, these were present in 21, slight in three, and very marked, with loss of consciousness, in four cases. Recovery took place in from a few minutes to an hour, treatment consisting in

lowering the head, injections of camphorated oil, atropine, and the administration of oxygen.

The author believes that it is probably not wise to administer morphia as a preliminary step or to use spinal anæsthesia in a patient with very low blood pressure because of the associated drop resulting from the anæsthetics which may be as great as 20 mgs. of mercury. Spinal anæsthesia is indicated, he thinks, especially in cardio-vascular patients, chronic respiratory cases, over-weight or high pressure individuals as these classes take inhalation-anæsthesia badly.

The surgical, medical and anæsthetic problems of the toxic goitre case with heart failure. F. H. Lahey *et al.*, Boston, Mass. *Amer. Jl. of Surg.*, Anæsthetic Supplement, July, 1925.

In a series of 70 operations at the Lahey Clinic it was found that while, theoretically, local anæsthesia was thought to be indicated because it puts practically no direct strain on the heart, actually, it was discovered that these patients were not of the type for which local anæsthesia was suitable. They later used nitrous-oxide-oxygen plus ether, or ethylene-oxygen plus ether. They conclude that for this particular type of patient ethylene-oxygen is safer because more oxygen can be used with it so that less ether is necessary, and because blood pressures, pulse-rate, and respiration are less elevated.

In preparation for the operation patients were given a minimum of three weeks' rest in bed, during which time, with four exceptions, all were thoroughly digitalized, the excepted cases being the ones who did not have auricular fibrillation.

Scopolamin-apomorphia amnesia in criminology. R. E. House. *Researches in Anæsthesia and Analgesia,* Avon Lake, Ohio, June, 1925.

Dr. House's method, known popularly as *Truth Serum,* is a form of *Third Degree* recommended by him for forcing the truth from criminals or alleged criminals by means of scopolamine-apomorphia, injected subcutaneously. It is a method he has been enunciating for some time now, he himself claiming 50 per cent. successes.

His belief is that memory can be extracted, against the will, from an individual's subconscious mind by injecting proper doses of these drugs, which tend to destroy temporarily all the functions of the brain except the activity of the hearing centre. This *Receptive* or *Examination Stage* may be maintained for as long as four hours, and during this time of artificial unconsciousness questions are put to the person examined which are answered truthfully because, though in a condition of profound sleep or wakefulness, he is *without the ability*

to reason, the answer being stored in the mind as pure memory. Replies are given without evasiveness, guile, deceit or fraud because the drugs cause facts firmly implanted in the mind, which, for various reasons, are closely guarded under normal conditions, to be released.

In New Orleans recently the three daily newspapers obtained permission from the local Medical Society to have the test applied to three reporters. The tests were said to be successful.

In a paper entitled *Scopolamine-apomorphia amnesia in psychiatry*, in the same Journal, Dr. P. R. Vessie reports the case of a man who, desiring a passport to return to his home in Europe, persistently refused to give his past residences. Suspicion having been aroused he was detained and later placed in *scopolamine sleep*, during which he named four institutions in another country in which he had been an inmate, confessing that he had recently escaped from one. Inquiries made at these places proved the truth of his replies. Dr. Vessie, who is Senior Assistant Physician at the Gowanda State Hospital for the Insane, New York, tried the method extensively in that institution, and thinks that while it is comparatively a simple matter with the proper technique to communicate with a healthy mind during this sleep, the approach to a disordered mind is more difficult, but caution, patience and repeated examination will yield results sufficient to prove the value of this novel branch of psychiatric practice.

Alpha-lobelin as a specific respiratory stimulant: with special reference to its use in anæsthesia. Herbert S. Reichle. *Researches in Anæsthesia and Analgesia*, Avon Lake, Ohio, Aug., 1925.

This drug is the essential alkaloid of the *lobelia inflata* herb, first isolated by Heinrich Wieland, brother of Prof. Hermann Wieland, a recent contributor to this Journal. Its powerful and reliable action upon the respiratory centre makes it of interest to the anæsthetist. Puppel, quoted by the author, restored respiration within 30 seconds, in a case in which, after a combination of ethyl chloride, chloroform and ether anæsthesias, the patient had gone in a state of areflexia with pallor and maximal mydriasis. Sattler used lobelin to denarcotize. The author states that in cases where the respiratory centre, respiratory space and passage are still compatible with life, *i.e.*, where we have a functional depression, alpha-lobelin is a drug of great value for use in emergency. The drug has been used subcutaneously, intramuscularly and intravenously, the latter in extreme emergency. Wieland's doses are: for adults one-sixth grain subcutaneously and one-twentieth grain intravenously.

Major urological operations under regional anæsthesia. Oswald Swinney Lowsley. *Boston Med. and Surg. Jl.*, Boston, Mass., Sept. 3, 1925.

Lowsley reports 279 urologic cases performed under sacral, para-sacral and para-vertebral anæsthesia. In this series he demonstrated that operations on every organ in the genito-urinary system are practicable under regional anæsthesia. He thinks it a special advantage that by this method the patient may take fluids up to, during and immediately after operation; that the blood pressure is not elevated as it is in any type of inhalation anæsthesia, or depressed as it is in spinal, therefore there is very little bleeding during the operation; that the anæsthesia persists for many hours, thereby eliminating pain.

The supreme disadvantage is that what is injected cannot be extracted, therefore it must be given slowly, and one must stop at the slightest evidence of toxicity.

Excision of the perineal body of a monkey under intra-arterial anæsthesia. J. P. Arland. *Indian Med. Gazette*, Calcutta, Aug., 1925.

Twenty cc. of a 2 per cent. solution of gum acacia containing one-fourth grain of cocaine hydrochloride, along with 5 minims of adrenalin hydrochloride (1 in 1,000) were slowly injected into the lumen of the collapsed right common carotid artery. After five minutes the head and face were found to be completely anæsthetic. The anæsthesia was very sound. The common carotid artery was clamped to shut off the blood current in order to provide a bloodless field for the action of the anæsthetic, for the presence of blood has a retarding influence on the power of the anæsthetic solution.

The addition of gum acacia combined with adrenalin increases the duration of the anæsthesia. The internal jugular vein was clamped in order to lock in the injected solution as far as possible.

Administration of ethylene-oxygen. John S. Lundy, Mayo Clinic. *Iowa State Medical Society*, July, 1925.

Dr. Lundy thinks that there is, at present, no other anæsthetic agent which affords as profound anæsthesia with so little reaction as does ethylene when it is properly given. Its advantages are most strikingly demonstrated in the weak and poor risk patient.

In its administration it is found that the patient will occasionally hold his breath reflexly, as sometimes happens with open ether. Lundy administers at this stage a little carbon dioxide with the ethylene and oxygen, or ethylene, oxygen and ether, as the case may be. He has tried this in 400 cases with such good effect that he believes that this gas will be extensively used in the future. Carbon dioxide, he declares, makes general anæsthesia easier and gas anæsthesia more efficient, as respiration is more easily controlled.

Operation shock and anæsthesia in gastric surgery. R. Simon and R. Fontaine. *Revue de Chirurgie,* 1925, No. 2, p. 138 *et seq.*

Patients sometimes die as the direct result of abdominal operations, however well these have been conducted, and it is incumbent upon the surgeon to determine and prevent if possible such disasters. The sequelæ developing during or immediately after operation are the factors which constitute "shock," and those which occur after an interval of a day or two are generally complications of toxicity or sepsis. The post-operative circulatory depression may lead to œdema of lungs easily rendered fatal when aggravated by sepsis or by any interference with the normal movements of respiration.

As regards the origin of pulmonary infections one cannot ignore the injury caused by general anæsthetics, but that these are not the sole factors has been shown by the statistics of Gottstein, who experienced the same proportion of pulmonary morbidity and mortality under purely local as under general anæsthetics.

American surgeons lay stress on the importance of looking for foci of sepsis in the mouth and the naso-pharynx.

Lambret, Pierre Duval, Roux and Montier hold that an ulcer of the stomach may be a source of pulmonary sepsis owing to microbes being set free by manipulation of the organ during operation.

It will be agreed that all the conditions due to sepsis are better understood than are those which constitute pure shock, and it is this phenomenon which has formed the basis of our experiments and observations made on rabbits, dogs and man.

Our first series of experiments confirm the statements of others, that when the abdomen is opened without anæsthesia, the skin incision causes a rise of blood pressure, and manipulation and evisceration cause a fall varying with the manipulation, but progressive in nature. The fall often registered 5 or 6 centimetres, and the pressure slowly returned to normal when the abdomen was closed. This rapid fall of pressure is produced more readily by pulling upon viscera which are fixed than upon those which are more movable; for instance in dogs it was often the case that the stomach and most of the liver could be delivered without effort, and traction of such flaccid organs led to only slight fall of blood pressure.

The same results were obtained in dogs previously injected with morphia. The slow and continuous fall of pressure associated with opening of the belly is due to vaso-dilatation of splanchnic origin, and accompanies all abdominal operations of any duration and tends to recover when the peritoneum is closed. On the other hand, the sudden fall caused by traction on viscera has all the characteristics of a reflex, and the tracings show a great similarity to those obtained on division of the vagus. In five rabbits and two dogs we cut the vagus in the neck with the result that the blood pressure immediately rose but laparotomy and traction were still capable of causing a fall.

After section of the spinal cord at 3rd D.V. in one rabbit and at 6th D.V. in another, traction on viscera caused the usual phenomena. But in a rabbit in which the nerves of the œsophagus were divided, and in a second rabbit where the nerves of the lesser curvature of the stomach were cut, death quickly followed upon extreme fall of pressure. Section of the splanchnics, in two dogs, did not prevent the fall of pressure caused by traction on viscera.

In order to study the influence of anæsthesia upon this reflex the following observations were made, the methods employed being ether, chloroform, 1 per cent. cocaine, and anæsthesia of splanchnic.

General anæsthesia caused initial rise of pressure followed by fall, which was aggravated by irregularity of administration; in two cases where pressure had been reduced to 0, intracardial injection of adrenalin was successful in re-establishing it. Under general anæsthesia there is practically no rise of pressure when skin is incised, and the subsequent slow fall is modified, and the rapid fall incident upon traction and manipulation is less in degree.

Infiltration of mesentery with cocaine obviated the fall of pressure due to traction as was well illustrated in a case where only the gastric mesentery had been injected—traction on the stomach causing no fall whereas stretching the pedicle of the kidney immediately caused a fall.

Anæsthesia of splanchnics from the front in a dog in no way altered the reflex. Our conclusions are that general or local anæsthesia cannot control the circulatory disturbances but can modify depression due to reflex causes, and that local anæsthesia can actually prevent these reflex phenomena completely.

Blood pressure observations were made in 19 cases of gastro-intestinal surgery, seven resection of stomach—one of which was performed under parietal infiltration only—three under parietal and mesenteric infiltration, one under splanchnic anæsthesia, and two under parietal with ether. Five gastro-enterostomies—one under ether, three parietal only, one parietal and general. Two duodeno-jejunostomies, both under general anæsthesia. Four explorations—two under ether, one with parietal infiltration only, one parietal and general. One gastrostomy under parietal infiltration.

Our researches demonstrated that when viscera had to be manipulated and pedicles pulled on, the following conclusions were indisputable :—

General anæsthesia resulted in less shock than parietal infiltration alone.

When general anæsthesia is not employed parietal infiltration, together with infiltration of mesentery or of splanchnics was superior to infiltration of parietes alone.

Our experiments have also proved very clearly that where local anæsthesia is employed it is of the utmost importance that all mani-

pulations, especially traction on pedicles, shall be performed with the utmost gentleness.

Although lung complications were less common in our experience with local than with general anæsthesia, it is not primarily on this account that we advocate its use but rather with a view to lessening shock. A. L. FLEMMING.

Anæsthetic reflections. Elie Faure. *Pressé Medicale,* Aug., 1925, No. 63.

Before the Great War patients for anæsthesia came under four headings: 1, normal; 2, nervous; 3, alcoholics and drug takers; 4, neurotic alcoholics and drug-takers. The author estimates the proportions as 40 per cent. normal, 50 per cent. nervous, 3 per cent. alcoholics, 2 per cent. neuro-alcoholics. Since the war a generous computation would give—5 per cent. normal, 20 per cent. nervous alcoholics and alcoholics, and 75 per cent. nervous, which means that the anæsthetist has much more nervous excitability to contend with than formerly.

Men of about 30 years of age constitute almost entirely the class of neuro-alcoholics; men and women under 20 and over 40 constitute the nervous group. The 5 per cent. normal are principally young girls or women between 20 and 30—in whom the ill effects of dissipation is counteracted by healthy sports and open air life.

Before, during and after anæsthesia excitement is much more prevalent now than was the case before the war.

Before anæsthesia—more obvious fright; affected courage; want of calmness.

During induction—more hysterical laughing, crying and sudden movements (often just as a patient seems about to fall asleep); more holding of breath and cyanosis, more nausea and vomiting.

After anæsthesia—there is more commonly vomiting and noisy awakening with increased call for morphia.

During anæsthesia Faure found, when using Schleech's mixture, a frequent instability in the size of the pupils, often passing from myosis to dilatation in a few seconds without noticeable change in respiratory or cardiac rhythm, or in the general condition of the patient—signs which might well alarm an anæsthetist who was not prepared for them.

One ought to be on one's guard with patients who affect calm or even gaiety, in whom there are often noticed verbosity, fine tremor, rapid pulse and erythematous rash on chest and neck after the commencement of induction—these patients often very suddenly become violent.

Excitement in the simply nervous patient is easily calmed by steady induction, but in alcoholic subjects induction may be almost impossible without great risk—a slight excess in the dose causing syncope and too weak a vapour allowing the patient to "come round."

It is in the neuro-alcoholics that signs of anæsthesia are most perplexing—irregular pupillary signs, contractions and jerkings of muscles, trismus, falling back of the tongue and cyanosis. In fact those difficulties which existed before have become so increased since the war as to render the task of the anæsthetist much heavier

A. L. FLEMMING.

The Borocaines: a new class of local anæsthetics. A. J. Copeland and H. E. F. Notton. *Brit. Med. Jl.*, Sept. 26th, 1925.

These were developed in an effort to find a local anæsthetic equal in efficiency to cocaine hydrochloride but without its poisonous properties.

The borates of the local anæsthetic bases—ethocaine, tutocaine, butyn, "G.S.," cocaine, beta-eucaine, stovaine, alypine and holocaine borates—possess entirely specific properties as surface anæsthetics, and these are classed under the heading of "borocaines." Their efficiency depending upon the pH value for the salt in aqueous solution being kept high, great care has to be taken in their preparation. For this reason their manufacture has been entrusted to The British Drug Houses, Ltd., London, N.1, and this firm is prepared to supply the compounds.

De-etherization with carbon dioxide. Ethel Righetti. *Am. Journ. of Surgery*, Sept. 1925.

Dr. Righetti gives the results of the use of carbon dioxide in 46 children. She unhesitatingly recommends its use where rapid ventilation of ether is required owing to the increase of the respiratory rate and volume caused by it. Another advantage is that consciousness can be established within a short period following cessation of the anæsthetic. In all but two cases respirations became deep and rapid, the colour was a good pink. In these, however, two small children became livid and the Co_2 was stopped at once. Post-anæsthetic nausea and vomiting are lessened.

Dr. H. E. G. Boyle, Lecturer on Anæsthetics, St. Bartholomew's Hospital, London, has been recognised as a teacher in Anæsthesia by the University of London.

The Dental Board of the United Kingdom have arranged for a series of lectures on General Anæsthesia to be given at the Manchester University, Manchester. Prof. Yandall Henderson, of Yale University, will speak on Respiration in Anæsthesia, and The Absorption and Elimination of Volatile Substances through the Lungs, on Oct. 26th and 27th. Prof. Leonard Hill will deliver an address on The Circulatory Changes in Anæsthesia and the Use of Oxygen, on Nov. 3rd, and Prof. Noel Paton's subject will be The Metabolic Changes in Chloroform Poisoning, Jan. 16th, 1926.

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